JOURNAL

OF

THE FRANKLIN INSTITUTE

OF THE STATE OF PENNSYLVANIA,

FOR THE

PROMOTION OF THE MECHANIC ARTS.

NOVEMBER, 1861.

CIVIL ENGINEERING.

Suspension Girder Bridges for Railway Traffic.

From the Lond. Civ. Eng. and Arch. Journal, Nov. and Dec., 1860.

THE construction of four new bridges across the Thames, completed or in process, in the short distance between Hungerford Pier and Chelsea Reach, and designed on the several systems of suspension chains, arch, arch-girder, and girder, has the effect of bringing obviously before the eye of the public the working out of different theories of construction, which would otherwise be rather of a scientific or professional than of a general interest. It may therefore prove not unseasonable, nor perhaps wholly unprofitable, to dwell a little on the subject handled by Mr. P. W. Barlow, F. R. S., in his paper read before the British Association at Oxford.

Is it expedient, in the construction of bridges of large span for railway traffic, to employ chains, relying on a girder platform to reduce within safe limits the wave that inevitably accompanies the transmission of a rolling load over a common suspension bridge?

The question has been put to a practical test in Mr. Roebling's railway bridge over the Niagara; where a span of more than 800 feet is crossed by means of wire chains supporting a roadway and railway platform; the two platforms being so connected by means of trussing as to form a deep girder. In traversing this bridge the trains proceed at a foot-pace, and the accounts which reach this country as to the

degree of disturbance caused by their transit are somewhat conflicting. Its engineer is, however, understood to be as fully satisfied with the steadiness of the structure as he has every reason to be with the remarkably low cost. There is an additional safeguard against undulations in this bridge; cables being attached under the platform, and anchored to the rock below, thus serving as powerful stays: an excellent precaution, but one for which there would be no facilities in the majority of cases. A peculiar feature is the employment of a distinct set of chains for each platform; the deflection of one pair of chains being thus considerably greater than that of the other. It would follow that on any considerable change of temperature, the chains supporting the upper platform would naturally rise and fall appreciably more than those supporting the lower one; and as these platforms are firmly trussed together, it does not appear in what way the danger is avoided of an undue proportion of the weight of the entire structure being occasionally thrown on one set of chains. The extension of the chains when a heavy load comes on the bridge would tend to bring about a similar result.

There are, therefore, special circumstances about this bold and original work which prevent its being taken as a pattern or sample of suspension railway bridges; and very precise data must be obtained by means of careful observations before any general practical conclusions can be drawn from the success of the Niagara Bridge.

The question, then, of the suitability of suspension bridges with rigid platforms to the requirements of railway traffic, is as yet to be regarded as a matter of opinion; and there are few points on which the opinions of engineers of the greatest eminence and talent are less agreed than on the one now under consideration. The two master-spirits, who have left lasting monuments of their constructive genius in the railways of Great Britain, have strikingly exemplified the divergence of their views, in the bridges erected by the one over the Menai, the Conway, and the St. Lawrence; and, by the other, over the Wye and the Tamar.

The enormous strength and rigidity of the Menai tubes is such, that the addition of suspension chains would be simply useless, if not even detrimental. For the expansion and contraction due to extreme changes of temperature would cause the chains to rise and fall midway between the piers, to an extent to which the tube would not readily adapt itself; so that the main weight of the whole structure would sometimes be carried by the chains and sometimes by the tube. It was well known that Robert Stephenson was no advocate for the use of chains for railway bridges in any case. But, conclusive as the consideration just stated is against the combination of a chain with a beam of the proportionate strength of the Menai tube, it does not apply to a suspension bridge with a platform flexible enough to rise and sink as expansion or contraction may require; and at the same time stiff enough to insure the equable distribution of any partial load over the whole length of the chain.

The bridge over the Wye at Chepstow, and the very similar but

more colossal structure over the Tamar at Saltash, have much in common with the description of stiffened suspension bridge so strongly recommended by Mr. Barlow; but are distinguished by two main points of difference. In the first place the tension of the chains is sustained by straining-tubes resting on the summits of the piers, instead of by the ordinary stay-chains; in the second place, the platform girders are very slight, and the wave is prevented mainly by the introduction of iron trussing in the space between the tube and the chain.

Similar diversity of views may be seen exemplified at Chelsea and Hungerford. The massive chains of the new Chelsea Bridge, and the strong *gridiron* arrangement of the suspended girders, defy all the shocks and disturbances of common wheel traffic. At Hungerford a girder bridge is in course of construction, which would exhibit as marked a contrast to the fine suspension bridge of Brunel, as yet undisturbed, as the Britannia tubes present to the chains of Telford, at Menai. This new work seems to evince, on the part of its distinguished author, a persuasion that the use of chains is inadmissible under any circumstances in railway bridges; for, had it been thought practicable and safe to throw a suspended girder across the span of 670 odd feet, the existing piers would have presented singular facilities; the extent of the foundations being sufficient for a greatly increased width of roadway.

On the other hand, Mr. Barlow records a no less decided judgment on the part of Sir W. Cubitt and himself, that a simple suspension bridge, with a tubular girder roadway, may be traversed by railway trains with perfect safety and small disturbance.

It is therefore sufficiently clear, that if we consult the opinions even of the men most competent to pronounce on such a question, we are led to very opposite conclusions; and until the experiment is actually made on a large scale in some railway bridge, our only remaining resource lies in experiments on models, or in theoretic deductions.

The economy of the ordinary suspension bridge is mainly due to the property possessed by chains of adjusting themselves to any curve of equilibrium required by a new disposition of the load.

In the arch, if a load becomes so distributed as to throw the line of pressure near the edges of the voussoirs, the tendency of the thrust is to cripple the arch and destroy equilibrium. As a rolling load traversed the bridge, the line of pressure (in which the thrust is considered to be transmitted from the crown to the abutments) would, if it could be rendered visible, be seen to waver and distort itself. A bulge or swell would travel along it, raising it perceptibly at the successive points over which the load passed; and elsewhere it would be depressed and flattened. To insure the stability of an arch under all possible conditions, it is therefore necessary to give a far greater depth of voussoir, and consequently to employ a much larger amount of material than would be demanded simply to sustain the thrust. In girder arches there is a similar necessity for an excessive amount of material, either in the bow itself or in spandrels or trussing, to arrest change of form.

The effect of tension, on the other hand, is to guide the suspension chain into its position of stable equilibrium; so that if only sufficient play is afforded for spontaneous adjustment, no more material need be used than is required to sustain the simple strain of tension. A second advantage possessed by suspension over other wrought iron bridges results from the whole of the metal being employed to resist a tensile strain, no part being in compression. This causes a saving of material, on account of the strength of wrought iron to resist extension being considerably greater than its strength to resist compression. A third source of economy is one also shared in some degree by arches, and lies in the deep versed sine which can be given to the curve of the chains. The greater the versed sine, or rise, of the chain or arch, the less is the tension or thrust, and consequently the less is the material necessary to sustain it. An analogous reason renders it advantageous to increase the depth of a girder to a certain extent, but within far narrower limits; as the necessity of providing for lateral stiffness as well as the weight of webb or trussing absorbs much of the saving that would otherwise be effected in making girders very deep. A fourth advantage in favor of the suspension bridge is, that the reaction which maintains the tension of the chains is furnished by stay-chains attached to their extremities; while in the girder an upper flanch must be provided, the compression of which reacts against the extension of the lower flanch, giving a double amount of material in flanches alone, as compared with the chain. To all these reasons for the great economy of suspension bridges, add the important consideration that in considerable spans the weight of the bridge itself begins to bear a large proportion to the total load, a proportion which rapidly augments as the length of bridge is increased. It is thus that for spans of 300 or 400 feet the cost of the girder becomes enormously greater than that of the chain, and that suspension bridges can be thrown over spans utterly impracticable for any other kind of structure. Of course, in any viaduct it is of primary importance to avoid large spans as far as practicable. But cases will occur in which it is difficult or impossible to do so. Must the railway engineer in these cases forego the advantages peculiar to the suspension bridge? Further, in positions where the least practicable span exceeds the possibilities of a girder bridge, is he to reject the only description of structure that will meet the requirements of the case? If it is in the power either of experiment or of analysis to throw any fresh light on this vexed question, the study cannot be deemed unimportant or uninteresting.

Great allowances are necessary in drawing any conclusions as to a proposed work of considerable magnitude from experiments on a model, however carefully conducted. And, in fact, it is almost essential for theoretic analysis to go hand in hand with experiment, to assure us that we are justified in accepting such conclusions as valid guidance for a great work, especially if of a novel character. On the other hand, in analysis, if the reasoning is sound, and if we have ground for believing that nothing has been omitted or misstated in the data, there is a fair presumption of the correctness of the result: and if

this result is found to agree with experiment, few will hesitate to admit its truth.

It is therefore now proposed to inquire, what solution theory affords to the problem of the suspended girder; and while doing this it may be useful to note how far the results arrived at accord with Mr. Barlow's experiments. It will then be endeavored to indicate some general practical conclusions. It might have been of great help in such an inquiry to have been made acquainted with the course of reasoning pursued by Prof. Rankine, and to know the process by which the learned professor arrived at the conclusion cited in Mr. Barlow's paper. In the absence of the guidance which such knowledge might have afforded, it is necessary to proceed as upon untrodden ground.

If a weight be placed on any part of a girder suspended from a chain, but having its ends kept from rising at the piers, the chain will be observed to be drawn down by the loaded girder, and the further parts of the chain will be drawn up, drawing with them the girder. The girder will thus assume an S bend, with an additional half-bend in cases when the weight is applied near the half-span. What is the law of this compound displacement? The reason that part of the platform is drawn up when part is deflected, is obvious. The chain is fixed at the ends, and is of a certain length: when a partial deflection takes place, the curvature of the portion of chain immediately affected is increased, and this has the effect of *taking the slack out* of the rest of the chain (the total length being unchanged) by flattening its curvature; which at the same time draws it up, and the platform with it.

First of all, then, the law must be ascertained by which the vertical displacement of any portion of the chain affects the horizontal measurement from end to end. This law is as follows:—Let the variable y be the ordinate of the curve of the chain; and let the vertical displacements of the various points be plotted in a curve, of which the variable z expresses the ordinate. z is therefore, in fact, the total vertical displacement of any point in the chain, and is supposed to be inconsiderable compared with the total rise of the chain.

Let x be the abscissa, or horizontal distance. Then $\int \frac{dy}{dx} dz$ gives the total alteration of the horizontal measurement between any two given points of the chain, consequent on the supposed vertical displacements.

If now the wave of displacement of the platform can be resolved into two—namely, first a wave of simple deflection, in which the whole girder would sink under its load if unsupported by the chain, and, secondly, a reflex wave, in which the reaction of the chain pulls up the girder—we shall find the following relation to subsist between these waves. Call their respective ordinates z_1 and z_2: then

$$\int \frac{dy}{dx} dz_1 + \int \frac{dy}{dx} dz_2 = 0;$$

the integrals being taken for the whole distance from tower to tower. Because, apart from the stretching of the chain, which it is unneces-

sary at this moment to take into account, any other result would involve an alteration of the span, or a break in the continuity of the chain.

For a girder of a given uniform section, the wave of simple deflection may be determined by the ordinary rules. As the resistance of the girder to vertical displacements will be far more powerful than that of the chain, the displacements being very small compared with the versed sine of the curve of the chains, no serious amount of error is involved in assuming that the form of this wave depends absolutely on the girder, without reference to the chains.

We can therefore at once give an algebraical expression for z_1, the ordinate of the wave of simple deflection. Thence can be determined the value of $\int \frac{dy}{dx}\, dz_1 = \mathrm{H}$, the horizontal distance by which the span of the chains would be diminished through the wave of simple deflection, were the effect not compensated by the reflex wave.

The reflex wave must be of a form due to the action of an equally diffused upward pressure on the girder, since the curve of the chains (when the displacement is not considerable) may still be considered to be that due to equal loading; and the equal loading of the chain being conveyed through the suspension-rods attached to the platform, the reaction on the platform must be equally distributed also. For similar reasons to those already adduced, the form of this reflex wave is deducible from the laws of girders simply. Its magnitude—and, in consequence, the proportion which the chains bear of the entire load—is determined by the equation

$$\mathrm{H} + \int \frac{dy}{dx}\, dz_2 = 0;$$

which gives the conditions on which alone the reflex wave can compensate the wave of deflection.

Having thus determined the simple wave of deflection and its ordinate z_1, and the reflex wave and its ordinate z_2; the superposition of the one wave on the other gives the actual compound wave of deflection with its ordinates $= z_1 + z_2$. The actual strain on the girder at any point will be found by subtracting from the strain accompanying the simple deflection the upward strain due to the reflex wave. In fact, the process of the analysis has regarded as successive two waves which are actually simultaneous; and by thus resolving the compound wave into its component parts, a solution is attained capable of simple and ready application.

If, for instance, it be inquired, what effect will be produced by a weight w placed on the suspended girder at a point distant d feet from the half-span (the ratio $\frac{d}{s}$ being expressed by r), it will be found, on pursuing the method of investigation just indicated, that a distributed pressure will be thrown on the chains

$$= \left(\frac{25}{16} - \frac{15}{2}\, r^2 + 5r^4\right) \times w;$$

and that the reflex wave due to the reaction of the chains will have its ordinate at the half-span

$$= k \times \frac{5}{6} \frac{s^3}{64} \left(\frac{25}{16} - \frac{15}{2} r^2 + 5r^4 \right) \times w,$$

k being a coefficient depending on the rigidity of the girder, and s being the span. Combining the reflex wave with the simple wave of deflection (determined by the usual rules for girders), in which the same coefficient k will occur, the compound wave is obtained. A correction for the effect of the stretching of the chains, which goes to reduce the reflex wave, remains to be made, and the result is a very close approximation to the actual state of the suspended girder under the assumed conditions.

The case in which a single concentrated load produces the greatest wave, and causes the greatest strain to be thrown on the girder, is when it is placed at a distance midway between the half-span and one of the towers. A load of 256 cwt. placed in this position causes a distributed pressure on the chains equal to 285 cwt. This excess of 29 cwt. is due to the reaction of the further end of the girder, which is supposed to be fixed down to the pier by rollers, as it would otherwise be lifted from its seat by the tension of the chains. Let us compare the wave caused by a weight in this position with the deflection which the same weight would produce if placed in the centre of the girder, the support of the chains being withheld: w being the weight, and k the constant already referred to, $-kw \times \frac{s^3}{48}$ is the deflection which would follow were the girder not suspended, and the weight placed in the centre. To facilitate comparison we will therefore express the ordinates of the wave now under examination in terms of $\frac{kws^3}{48}$.

First, the simple wave of deflection will have for its ordinates,

at the loaded quarter-span, . . $-0{\cdot}5625 \times \frac{kws^3}{48}$

at the half-span, . . . $-0{\cdot}6875 \times \frac{kws^3}{48}$

and, at the opposite quarter-span, . . $-0{\cdot}4375 \times \frac{kws^3}{48}$.

The reflex wave will have for its ordinates,

at the loaded quarter-span, . $+0{\cdot}4959 \times \frac{kws^3}{48}$

at the half-span, . . . $+0{\cdot}6958 \times \frac{kws^3}{48}$

at the opposite quarter-span, . $+0{\cdot}4959 \times \frac{kws^3}{48}$.

The addition of these ordinates will give the following as the ordinates of the resultant wave:—

at the loaded quarter-span (a depression), $-0{\cdot}0666 \times \frac{k w s^3}{48}$

at the half-span (a rise), . . $+0{\cdot}0083 \times \frac{k w s^3}{48}$

at the opposite quarter-span (a rise), $+0{\cdot}0584 \times \frac{k w s^3}{48}$.

The greatest deflection is, therefore, $-0{\cdot}0666 \times \frac{k w s^3}{48}$, or $\frac{1}{15}$th of the deflection due to the same load placed on the middle of the girder without chains. This result has, however, to be increased by the correction for the stretching of the chains.

The preceding result cannot be directly compared with those given by Mr. Barlow's experiments, because the reflex wave in his model is affected by the inequality in the height of the towers, which is such as to increase this wave by $\frac{1}{7}\frac{1}{3}$d part when the load is placed one-fourth of span from the high tower, or to reduce it by the same amount when the load is placed one-fourth of span from the low tower. We will, therefore, take the mean of two experiments with the first girder, the load being 56 lbs.; in the first case, placed at quarter-span from the high tower, and, in the second case, at quarter-span from the low tower; and compare the mean so obtained with the deductions of theory.

The girder experimented on was one which, without the chains, deflected 1·2 ins. when a load of 42 lbs. was placed on the centre. In this instance, therefore, $\frac{k w s^3}{48} = \frac{1{\cdot}2}{42} \times w = \frac{w}{35}$. For the load of 56 lbs. at the quarter span we have, therefore, to multiply the co-efficients of $\frac{k w s^3}{48}$, as above determined, by the fraction $\frac{56}{35}$, or $\frac{8}{5}$, in order to obtain the ordinates, in fractions of an inch. The following is a comparison of the results:—

	Theory.	inch.	Experiment.	inch.
At loaded quarter-span, . .	Depression,	−0·107	Depression,	−0·110
At half-span, . . .	Rise,	+0·013	Depression,	−0·010
At opposite quarter-span, . .	Rise,	+0·093	Rise,	+0·055

With a load of 168 lbs. placed at the quarter-span, taking the mean of two experiments as before, we get the following results:—

	Theory.	inch.	Experiment.	inch.
At loaded quarter-span, . .	Depression,	−0·320	Depression,	−0·320
At half-span, . . .	Rise,	+0·040	Depression,	−0·065
At opposite quarter-span, . .	Rise,	+0·280	Rise,	+0·170

In glancing at these figures, an exact correspondence is noticeable

between theory and experiment in the amount of depression at the loaded quarter-span. We must not, however, allow ourselves to be deceived by an identity of result, which arises from a compensation of opposite disturbing causes. It is evident that the form of wave in the model is different from that of the calculated wave; that the centre is depressed instead of rising; and that, while theory gives 0·60 inch as the sum of extreme depression and extreme rise, experiment reduces this sum to 0·49 inch.

One element of this difference is to be sought for in the stretching of the chain in the model, which causes a deflection at the centre of the girder, and elongates and deepens the wave of depression, at the same time that it reduces the wave of elevation. The other element of difference lies in the resistance of the chain itself to a change of position, a resistance very noticeable in a heavily-weighted model with an exaggerated wave, but omitted in our theory; the error involved in the omission being one on the safe side. The operation of this cause in modifying and reducing the total wave (as measured from extreme deflection at one-quarter span to extreme elevation at the opposite quarter-span) may be traced in the successive experiments made by Mr. Barlow, in which the distributed load on the platform was gradually increased to 193 lbs. In this set of experiments, the girder was a plank, 7½ ins. by ¾ in., the deflection of which, without the chain, was 1·48 ins. with 10 lbs. A weight of 56 lbs. was placed at quarter-span from the high tower. The displacements according to theory, after correction for the unequal height of the towers, should be as follows:—

	inch.
At quarter from high tower,	— 0·496
At half "	+ 0·148
At three-quarters "	+ 0·540

The actual displacements with no distributed load on the platform were,

	inch.
At quarter from high tower,	— 0·48
At half "	— 0·02
At three-quarters "	+ 0·29

But with a distributed load of 193 lbs. placed on the girder, the displacements caused by the 56 lb. weight became reduced to the following:—

	inch.
At quarter from high tower,	— 0·31
At half "	+ 0·05
At three-quarters "	+ 0·20

As might have been expected, the more the wave is magnified in the model, by reducing the rigidity of the girder, the more widely the actual displacements differ from those of theory; owing to the resistance of the chain to these exaggerated displacements becoming more appreciable. Thus, for the girder which deflected without the chains 2·375 ins. under a load of 8 lbs., if we take a mean between the results given by 56 lbs. placed at one-fourth from the high tower,

and the same weight placed at one-fourth from the low tower, we find the calculated and the actual displacements to be respectively as follows:—

	Theory. inch.	Experiment. inch.
At loaded quarter-span, . .	— 1·108	— 0·855
At half-span, . .	+ 0·138	+ 0·160
At opposite quarter-span, . .	+ 0·971	+ 0·780

The result of the experiments in this instance is very diverse from those of the preceding experiments, especially in the proportionate largeness of the wave of elevation, which in fact seems anomalous.

On the whole, the comparison of these experiments with the results of calculation considerably confirms the accuracy of the theoretical formula, as applied to the small actual displacements of a full-sized bridge. It must, however, be remembered, that it has been assumed that the flexibility of the girder is everywhere uniform, and the same for upward as for downward pressure—conditions not always found in a plank, such as took the place of a girder in Mr. Barlow's model; and assumed, further, that the girder is not continued nor weighted for any distance beyond the face of the piers.

According to the calculated co-efficients, a concentrated load at quarter-span from one tower will cause a depression of 0·0666, or $\frac{1}{15}$th the deflection which the same load without the chains would cause if placed at the centre of the girder. This depression will be accompanied by a rise at the opposite quarter-span of 0·0584. The depression will be increased, and the rise diminished, by the stretching of the chains. The sum, which will not be affected by the stretching of the chains, is 0·1250, or $\frac{1}{8}$th.

(To be Continued.)

MECHANICS, PHYSICS, AND CHEMISTRY.

Ancient Standard of the British Inch. By JOHN TAYLOR.

From the Lond. Athenæum, June, 1860.

Sir John Herschel remarks, in the *Athenæum* of April 28th, that the British inch puts us in easy possession of "a modular system, which might be decimalized, and which, abstractedly considered, is more scientific in its origin, and, numerically, very far more accurate than the metrical system of our French neighbors."

I rejoice at this communication. It has saved our country from the introduction of the French metre, and from the admission of its superiority to our own measures, which, there is reason to believe, have been preserved, without any deviation from the standard, for about four thousand years.

The British inch had its origin, as I contend, in the measurement of the earth made by the founders of the Great Pyramid. They determined, with great exactness, the proportion which the diameter of a

circle bears to its circumference, and having ascertained the measure of the circumference of the earth, supposing it to be a perfect sphere, they divided the diameter into 500,000,000 of units, which we call inches. This appears to have been the origin of our inch. The *polar* diameter of the earth, according to Mr. Airy's calculation, is equal to 500,491,440 of these inches, which measure so little exceeds the mean diameter of the earth, according to the ancients, as to require the addition of only one-thousandth part, to render it, with all but mathematical precision, the 500-millionth part of the earth's axis of rotation.

Sir John Herschel says, that his attention was drawn in the first instance to this *rapprochement* by my statement, that the diameter of the earth, in the latitude of the Pyramid, is 500,000,000 of English inches; "which it is not:" and he adds, "It is singular that the reduction of Mr. Airy's polar axis from feet to inches, in p. 87, which is rightly performed, does not appear to have suggested the least misgiving as to the correctness of the statement." It appears to me that Sir John has misapprehended my meaning. My reference is to the *ancient* measure, and not to the *modern*—to the *mean* diameter, as it was then supposed, and not to the *polar*, as it is now estimated. I fear, however, that by my inadvertence in writing "average" for "polar," at p. 87, when I was comparing the modern diameter with the ancient, I have caused this misapprehension. He is surprised also that I did not "notice the important practical facility of reduction from the parliamentary to the modular standard." But this was not within my province. The proposal of a modular inch comes with great propriety from Sir John Herschel; and, having his recommendation, it will no doubt be adopted by scientific men, in those cases which require its use. The parliamentary inch will still be our measure for all practical purposes.

I agree with Sir John Herschel, that the founders of the Great Pyramid were not likely to be in possession of any *calculus* by which they could determine the true ratio which the circumference of a circle bears to its diameter, so as to be able to represent it with mathematical accuracy, in the proportion which the periphery of the base of the Great Pyramid bears to its radius. They were guided probably in their proceedings, by those general principles which would influence men of common sense. Hence, they may have supposed that if a Pyramid of a certain angle (51° 51′ 14″) had the property of representing the proportion which the radius of a circle bears to its circumference; and a Pyramid of another angle (51° 49′ 46″) had the property of representing the proportion which the square of its height bears to the content of one of its triangular faces; then a Pyramid of an angle between the two (51° 50′ for instance) might combine the two properties so nearly, as to make them think they could embody both results in one structure. They were completely mistaken in this conclusion, since what may be affirmed of the one angle cannot be affirmed of the other; but for want of a calculus they might not be aware of this impossibility.

An evident error in Herodotus led me to observe, with a view to its

correction, that the *square* of the height (though not the *height*) would be so nearly equal to the content of one of the sloping triangles, as to render it probable that the true reading of the passage might be found in this correction. Sir John Herschel approves of the suggestion. He says, "This is the characteristic relation which Herodotus distinctly tells us it was the intention of its builders that it should embody, and which we now know that it did embody, in a manner quite as creditable to their workmanship as the solution of such a problem was to their geometry. This problem, however, has no relation to that of the rectification of the circle." Certainly not. It is gratifying to me, that my conjectural emendation should be allowed to have restored the true reading with so much apparent certainty, after it had been so long lost to the world.

At the close of his communication, Sir John Herschel does me the favor to notice "another curious and novel relation" which I had pointed out at page 37 of "The Great Pyramid," and which "is interesting," he says, "as offering the only tolerable approach in round numbers to an arithmetical relation between any of the dimensions of this Pyramid and those of the earth." But in the *Athenæum* of May 5, he observes, "There is another and a remarkable one which I do not find noticed by Mr. Taylor." If he will refer to pp. 26 and 27, he will see that I have not overlooked it. In correcting the error of Eratosthenes (that the circumference of the earth is equal to 31,500 Roman miles), I say that it is equal to 27,000 miles, and that the tenth of a Roman mile is the height of the Pyramid, including the casing, and supposed to terminate in a point. Thus it is one-270,000th part of the earth's circumference, as Sir John Herschel has stated.

Leonard Place, Kensington, May 29.

The use of Ozone for Cleaning Books, Removing Ink, &c.

From the Lond. Mechanics' Magazine, July, 1861.

Gorup-Besanez* recommends the use of ozone for cleaning and restoring the coloring of old spotted and soiled books and prints. Ozone completely removes writing ink; but printing ink is not attacked by it, at any rate to no perceptible extent; grease spots and mineral colors also remain unchanged, but vegetable colors are completely removed. The method used is as follows:—The air in a sulphuric acid carboy is ozonized by Schönbein's method, which consists in placing in it a piece of phosphorus three inches long and half an inch thick, and pouring into the carboy as much water at 30° C. as will half cover the phosphorus; the carboy is loosely corked and allowed to stand in a moderately warm place until the air is charged with ozone, which generally requires from twelve to eighteen hours. Without removing the phosphorus and water, the article to be bleached is uniformly moistened with distilled water, and after being rolled up is suspended by a platinum wire in about the centre of the carboy. The roll of paper is soon seen to be continually surrounded by the

* Liebig's *Annalen*, May, 1861.

column of vapor rising from the surface of the phosphorus. The time required for the bleaching depends on the nature of the substance, but never requires more than three days; paper brown with age, and colored with coffee spots, in two days was quite white and clean. If the paper were now dried, it would not only be very brittle, but would also rapidly become brown; hence the acid must be completely removed. The paper is immersed in water, which is frequently renewed, until it only gives a very feeble acid reaction with litmus. It is next placed in water to which a few drops of soda have been added, and then, being spread on a piece of glass and placed in an inclined position, is exposed to a thin stream of water for twenty-four hours. After being allowed to stand till nearly dry, it is carefully removed, and dried between blotting-paper. Gorup-Besanez found that ozone was not well adapted for cleaning oil colors.

A new mode of Obtaining a Blast of very High Temperature in the Manufacture of Iron. By Mr. E. COWPER.

From the Lond. Athenæum, July, 1860.

Mr. E. Cowper read a paper descriptive of "A New mode of obtaining a blast of a very High Temperature in the Manufacture of Iron." The blast is obtained by an adaptation of the principle of Siemens's regenerative furnaces. A hot blast of a temperature of 2000° Fahr. can readily be obtained, and this without the destruction of iron tubes —the substance used in contact with the air being the most refractory fire-brick. This mode of obtaining a blast was in successful operation at Messrs. Cochran's iron-works. The temperature of the blast could be regulated to any required degree. The heat might be obtained from the combustion of the waste gases of the furnaces, and with greater economy than by any method hitherto known for economizing these gases.

Proceedings of the British Association.

For the Journal of the Franklin Institute.

Resistance of Wrought Iron Tubes to External and Internal Pressure. Deduced from Experiments of W. Fairbairn.

By CHAS. H. HASWELL, C. E.

No. 1.

In order to save space and to increase the generative powers of boilers, internal flues and tubes have been generally adopted, and without sufficient attention to the proportions of their diameter, length, and thickness of plates, so as to insure safety, and economy of material in its judicious distribution. Hitherto it has been considered a rule among engineers, that a cylindrical tube, such as a boiler flue, when subjected to a uniform external pressure, was equally strong in every part, and that the length did not affect the strength of a tube so placed. Although this rule may be true when applied to tubes of indefinite lengths, it is very far from true where the lengths are restricted within certain apparently constant limits, and where the ends are securely

fastened, as in heads or tube sheets, which prevent their yielding to an external force, or where, as in flues constructed in courses, the laps present a ring which greatly increases their resistance.

In some experimental tests to prove the efficiency of large boilers, it was ascertained that flues 35 feet long were distorted with considerably less force than others of a similar construction 25 feet long. This result led to further inquiry, and the following series of experiments were instituted, with very conclusive results:

RESULTS *of Experiments on the Resistance of Wrought Iron Tubes and Flues to External Pressure or Collapse.*

Welded Tubes, and Ends Secured to Head Plates.

No. of experiment.	Diameter.	Length.	Thickness of plates.	Pressure of collapse per sq. inch.	No. of experiment.	Diameter.	Length.	Thickness of plates.	Pressure of collapse per sq. inch.
	Ins.	Ins.	Ins.	lbs.		Ins.	Ins.	Ins.	lbs.
1	4	19	·043	170	11	6	30	·043	65
2	4	19	·043	137	12	8	30	·043	39
3	4	40	·043	65	13	8	39	·043	32
4	4	38	·043	65	14	8	40	·043	31
5	4	60	·043	43	15	10	50	·043	19
6	4	19	·043	140	16	10	30	·043	33
7	6	30	·043	48	17	12·2	58·5	·043	11·0
8	6	29	·043	47	18	12	60	·043	12·5
9	6	59	·043	32	19	12	30	·043	22
10	6	30	·043	52					

Riveted Tubes.					*Welded Tubes. Ends left open.*					*Riveted Flues. Ends closed.*				
No. of exp't.	Diameter.	Length.	Thickness of plates.	Pressure of collapse per sq. inch.	No. of exp't.	Diameter.	Length.	Thickness of plates.	Pressure of collapse per sq. inch.	No. of exp't.	Diameter.	Length.	Thickness of plates.	Pressure of collapse per sq. inch.
	Ins.	Ins.	Ins.	lbs.		Ins.	Ins.	Ins	lbs.		Ins.	Ins.	Ins.	lbs.
20	18·75	61	·25	420	23	4	60	·043	47	26	14·5 by 14·6875	60	·125	125
21	9	37	·14	262	24	4	30	·043	93					
22	9	37	·14	578	25	4	15	·043	147					

Cylindrical and Elliptical Riveted Flues.

No. of experiments.	Flues.	Diameter.	Length.	Thickness of plates.	Pressure of collapse per sq. inch.
		Ins.	Ins.	Ins.	lbs.
27	Cylindrical,	18·75	61	·25	420
28	do.	12·	60	·043	12·5
29	Elliptical,	20·75 × 15·5	61	·25	127·5
30	do.	14· × 10·25	60	·043	6·5

Tubes and Short Flues.

In the subjection of a tube or flue to external pressure, the material being compressed, becomes crumpled in longitudinal lines near the middle; the tube loses its original cylindrical shape at and near to that part, whilst the portions toward the extremities when supported by inflexible end plates, or the centre portions when sustained by the laps of the courses, retain their original form; so that the material virtually resisting compression is the comparatively small portion in the middle when the flue is of one course, or in the middle of each course when there are some two to four of them, and which, in the latter case, to a certain extent, is independent of the length of the tube, whilst the pressure producing the compression is always approximately proportioned to the area of the longitudinal section of the tube.

Hence, as the total external pressure on a tube or flue varies directly as its longitudinal section, that is, as the product of the length and the diameter.

$P' l d C = P$. *P′ representing the pressure to which the tube is subjected in pounds per square inch; l the length of the tube in feet; and C a constant to be determined.*

It has been ascertained by experiment, that the resistance of thin metal plates to a force tending to crush or to crumple them, varies directly as a certain power (x) of their thickness, the number indicating the power lying between 2 and 3.

Hence, the *Value* of a tube, &c., to resist collapse is as $\frac{P}{t^x}$; *t representing the thickness of the metal in inches.*

The mean of the product of $P' l d$ in the several experiments here given where the metal is of a uniform thickness of ·043 in., is 850, for a thickness of ·125 in., 9140, &c., &c., and the mean of the value of x for all thicknesses is 2·19.

Consequently, $\frac{850}{\cdot 043^{2\cdot 19}} = 835800$, and $835800 \times \frac{t^{2\cdot 19}}{l d} = P'$, which is the general formula for calculating the strength of wrought iron tubes and short flues subjected to external pressure within the limits indicated by the experiment; that is, provided their length is not less than 1·5 feet, and not greater probably than 10 feet.

In order to facilitate calculation, this formula may be written,

$$log.\ P = 1\cdot 5265 + 2\cdot 19\ log.\ 100\ k - log.\ (l\,d):$$

and by an obvious transformation,

$$\frac{850}{l\,.\,d} = P'.$$

By taking 2 instead of 2·19 for the index of t, this formula becomes as follows:

$$V \times \frac{t^2}{l\,d} = P', \textit{ the collapsing pressure.}$$

For thick tubes of considerable diameter and length, this formula is sufficiently exact for practical purposes.

v varies with the thickness of the tubes and flues, and may be safely estimated as in the following table:

When a Flue is constructed of courses, the above rule will apply by estimating the length of it to be the distance between the centres of two contiguous laps, if the whole length of the flue does not exceed three times the length of a course; when, however, the length does exceed that proportion, the estimate of its resistance is to be made by taking the units from the following tables:

In one experiment, the tube was divided into three parts by two rigid rings soldered upon its exterior, and its powers of resistance were thus increased in the ratio of three to one; *virtually*, the length was reduced in this ratio, and the strength was *actually* increased from 43 to 140 lbs. per square inch.

For Lengths from 1·5 *to* 10 *Feet.*

From	·043	to	·125	inch in thickness,	380,000 to 520,000.
	·125	"	·250	" "	520,000 " 650,000.
	·250	"	·375	" "	650,000 " 720,000.
	·375	"	·500	" "	720,000 " 800,000.

For Lengths from 10 *to* 18 *Feet.*

From	·125	to	·250	inch in thickness,	650,000 to 720,000.
	·250	"	·375	" "	720,000 " 810,000.
	·375	"	·500	" "	810,000 " 910,000.

For Lengths from 18 *to* 25 *Feet.*

From	·125	to	·250	inch in thickness,	720,000 to 810,000.
	·250	"	·375	" "	810,000 " 920,000.
	·375	"	·500	" "	920,000 " 1,020,000.

For Lengths from 25 *to* 35 *Feet.*

From	·125	to	·250	inch in thickness,	810,000 to 920,000.
	·250	"	·375	" "	920,000 " 1,020,000.
	·375	"	·500	" "	1,020,000 " 1,120,000.

NOTE.—In selecting the above units, regard should be had to the length of the flue, independent of the ordinary conditions of strength of the materials, and character of the riveting; as the nearer the length is to the limit of the length at the head of each table, the higher the unit is to be taken.

Illustrations.—1. Let $t = \cdot 043$ in., $l = 2 \cdot 5$ feet, and $d = 6$ ins.

Then, $\frac{\cdot 043^2}{2 \cdot 5 \times 6} \times 400{,}000 = \frac{\cdot 001849}{15} \times 400{,}000 = 49 \cdot 3$ *lbs.*

Experiments 7 and 10 give 50 *lbs.* for a length of but 2·5 feet.

2. Let $t = \cdot 25$ in., $l = 5$ feet, and $d = 18 \cdot 75$ ins.

Then, $\frac{\cdot 25^2}{5 \times 18 \cdot 75} \times 585{,}000 = 390$ *lbs.*

Experiment 20 gave 420 *lbs.* for a length of but 5 feet 1 inch.

3. Let $t = \cdot 375$ in., $l = 25$ feet, and $d = 42$ ins.

Then, $\frac{375^2}{25 \times 42} \times 920{,}000 = 123 \cdot 2$ *lbs.*

An experiment gave 127 *lbs.* for a length of 25 feet.

The following table will show how nearly this formula represents the results of the experiments on the different classes of tubes:

No. of experiment.	*d* Diameter.	*l* Length.	*t* Thickness.	P′ By experiment per sq. in.	P′ By formula of $t^{2\cdot19}$.	P′ By formula of t^2.
	Ins.	Feet.	Ins.	lbs.		
2	4	1·58	·043	137	130	130
5	4	5	·043	43	41	41
7, 10, 11	6	2·5	·043	55	54·7	54·7
13	8	3·25	·043	32	31·6	31·6
15	10	4·16	·043	19	19·7	19·5
18	12	5	·043	12·5	13·6	13·6
20	18·75	5·08	·250	420	407	435·5
21	9	3·08	·140	378	392	388·3
26	14·6	5	·125	125	116	134·7

RESULTS *of Experiments on the Resistance of Wrought Iron Cylindrical Tubes or Flues to Internal Pressure or Bursting.*

Number.	Diameter.	Length.	Thickness.	Pressure of rupture per sq. in.	REMARKS.
	Ins.	Ins.	Ins.	lbs.	
31	6	12	·043	475	Burst by rending of rivets.
32	6	24	·043	235	Burst through plates and rivets; plates very brittle.
33	6	30	·043	230	Burst by rupture of rivet-heads.
34	6	48	·043	375	Burst by rending of rivets.
35	12·13	60	·043	110	Burst through plates and rivets; plates brittle.

Formulæ of Resistance of Cylindrical Tubes or Flues.

The strain which the material of a cylindrical vessel is submitted to, when a uniformly-distributed external pressure is applied to it, is very different from the strain produced when the pressure acts internally. In the latter case the material is equally extended throughout all its parts, and its cylindrical form is preserved at all stages of the pressure, with the exception of the small portion of the plates when they overlap to close the extremities. The tube under a high internal pressure will assume the form of the middle frustrum of a spindle, and the relation of the force of rupture to that of resistance will be approximately expressed by

$$\frac{2\,\mathrm{T} \times t}{d} = \mathrm{P}.$$

T *representing the tensile resistance of the material per square inch in pounds,* t *its thickness,* d *its diameter in inches, and* P *the pressure requisite to produce rupture of the tube, or flue, in pounds.*

From a consideration of which experiments, it appears—

1st. That the resistance of Tubes or Flues to an External or Internal Pressure, varies directly and inversely as their diameters.

2d. That the resistance of a Tube or Flue to External Pressure up to the lengths experimented upon, is inversely as its length. Consequently, the resistance of tubes or flues to external pressure, of different diameters, but of equal lengths, varies inversely as their diameter, and contrariwise.

3d. The Tubes or Flues, with lap-joints, have one-third less resistance to external pressure, than when their joints are abutted.

4th. That a Cylindrical Tube or Flue has three times the resistance to external pressure of an Elliptical tube or flue, of the proportionate diameter given in the experiment noticed (29).

5th. That the length of Tubes or Flues, to resist Internal pressure, has no essential effect.

6th. That with Tubes or Flues of like thickness, their resistance varies inversely as the product of their lengths by their diameters.

Results of Experiments on the Resistance of Elliptical Flues to External Pressure or Collapse.

By comparing the result of Experiment (30) on the elliptical tube with the result of the experiments on the cylindrical tubes, we find that the preceding general formula will apply approximately to elliptical tubes, by substituting for d in that formula, the diameter of the circle of curvature touching the extremity of the minor axis. *Thus:*

$$\text{Diameter of the circle of curvature} = \frac{2r^2}{r'} = \frac{2 \times 7^2}{5{\cdot}125} = 19{\cdot}12 \textit{ ins.}$$

The pressure on this elliptical tube was 6·5 *lbs.*, which reduced to unity of length and diameter, = 621·4 *lbs.* (19·12 × 5 × 6·5), which result nearly agrees with 688 *lbs.*, the mean pressure of the 12-inch tubes also reduced to unity of length and diameter.

The pressure P′ per square inch, requisite to collapse a tube of variable curvature, varies inversely as the diameters of curvature.

(To be Continued.)

Notice respecting certain Phenomena of Crystallization and Polarization in Decomposed Glass. By Sir D. Brewster.

From the Lond. Athenæum, July, 1860.

At the meeting of the British Association held in Aberdeen, the author read a paper "On the Decomposed Glass found at Nineveh, Rome, and other localities," but not then having any drawings to exhibit to the Section, he found it difficult to convey an intelligible account of the structure and remarkable phenomena which the specimens exhibited both in common and polarized light. He now exhibited and explained very beautifully-executed colored drawings and diagrams explanatory of these appearances and properties. In this paper he omitted all reference to those colorless specimens by which he had then shown that a bundle or pile of these transparent films act upon

common and polarized light as negative uniaxal crystals, producing all the colors of polarized light, by the interference of two oppositely polarized pencils, one of which is the transmitted light, the other a combination of all the pencils reflected from the anterior surfaces of the films. He then pointed out the difference between artificial glasses and naturally-formed crystals, like rock crystal. In the glasses the atoms are forced, by melting them at high temperatures, to unite by chemical affinity. In the others the particles have united by peculiar polar actions while crystallizing naturally. Hence, the atoms of crystals being simple and similarly united throughout the entire crystal, have no tendency to decompose or reunite in other forms at particular parts; but the forces by which the earths, alkalies, and metals are composed, not being uniformly arranged as to the forces by which the different parts are held together, tend to separate and reunite in new or more natural crystalline relations in relation to particular points, lines, or surfaces in their mass. Thus the rock-crystal lens found by Mr. Layard at Nineveh was as perfect in its structure now as it was many thousand years ago, when in the form of a crystal, while the glass was found altered as in the specimens now shown; and few bodies cease to exist with such grace and beauty as glass, when it surrenders itself to time and not to disease. In stables, where ammonia and other exhalations prevail, and in damp localities, or where acids or alkalies prevail in the soil, the process is more rapid, and it may frequently be broken between the fingers of an infant, sometimes presenting in the middle a plate of unaltered glass, to which the process has not extended; but it is in dry localities, where Roman, Greek, and Assyrian glass has been found, that the process of decomposition is exceedingly interesting, and its results singularly beautiful. At one or more points in the surface of the glass the decomposition begins. It extends round that point in spherical surfaces, so that the first film is a minute hemispherical cup of exceeding thinness. Film after film is formed in a similar manner, till perhaps 20 or 30 are crowded into the 50th of an inch. They there resemble the section of a pearl (or of an onion), and as the films are still glass, the colors of thin plates are seen when we look down through their edges, which form the surface of the glass. These thin edges, however, being exposed to the elements, suffer decomposition. The particles of silex and the other ingredients now readily separate, and the decomposition proceeds downwards in films parallel to the surface of the glass; the crystals of silex forming a white ring and the other ingredients rings of a different color. Such is the process round one point, but the decomposition commences at several points; generally these points lie in lines, so that the circles of decomposition meet one another and form sinuous lines. When there are only two points near, these circles of decomposition surround the two points, like rings round two knots in wood; but when there are many points near, the curves unite and form sinuous lines. When the decomposition is uniform, and the little hemispheres have nearly the same depth, we can separate the upper film from the one below it; the convexities of the one falling into the concavities of the others.

The drawings of these were executed by Miss King, now the Honorable Mrs. Ward. When the decomposition has gone regularly on round a single point, and there is no other change, there is a division of the glass into a number of hemispherical films within one another. The groups of films exhibit in the microscope circular cavities, which, under different circumstances, become elliptical and polygonal. M. Brame, of Paris, succeeded in rapidly producing this decomposition by immersing glass in a mixture of fluoride of calcium and concentrated sulphuric acid, or by exposing it to the vapor of fluohydric acid.—(*Comptes-Rendus*, Nov. 2, 1852.) The author then went on, and with the diagrams explained the optical phenomena, grouping them into three chief varieties, but stating them to be so various and singular as to baffle description:—First, of those which have rough surfaces: these form an almost infinite number of hemispherical cavities on one side of the film, and similar convexities on the other. These are perfectly circular when separated by flat portions of the film; but when crowded together they are irregularly polygonal, the polygons forming a sort of network, the concave and convex surfaces not being rough, but specular, reflecting and transmitting white light, and exhibiting none of the colors of thin plates, but in polarized light acting as uniaxal crystals. Secondly, The second variety have perfectly specular surfaces, in consequence of having almost no cavities, in common light exhibiting in a very beautiful manner the colors of thin plates, the transmitted complementary to the reflected light. This variety is exceedingly rare. The specimen on the table showed blue as the reflected and yellow as the transmitted light. In some of the fragments a few insulated circular cavities with the black cross occurred, modified as to tints by the general tint of thin plates. Thirdly, The third variety consists of films containing cavities of all sizes and forms, from the thirtieth of an inch to such a size that they are hardly visible in the microscope, giving to the film a sort of stippled appearance. Their cavities are circular, elliptical, or irregularly polygonal, and they reflect and transmit complementary colors, some showing the black cross, though varied in its shape. The cavities are often arranged in sinuous lines, and encroach on one another. They frequently run in perfectly straight lines, and when very small and invisible as cavities, their margins form in polarized light brilliant lines, often grouped in bands like the stripes in ribbon; they are but a few thousandths of an inch in diameter, and might be used as micrometers in the microscope. These lines of polarized light all disappear when they lie in the plane of polarization of the incident light or perpendicular to that plane. Many other optical circumstances connected with this variety were mentioned by the author and explained. In all these three varieties the films are pure glass, for they become colorless by a sufficient inclination of the plates, and also by introducing a drop of water or alcohol, which, when it evaporates, allows the original colors again to be recovered, and although a film of the fluid separated each of the almost infinitesimal layers of the glass, yet they afterwards adhere as firmly as ever. If an oil or balsam be introduced, it slowly and un-

equally passes between the layers, so that the retreating color is bounded by a stratum of the various tints which the film combines. But the author has often found between the true glass films beautiful circular crystals of silex, finely seen in polarized light. These are sometimes dendritic, and assume, round the black cross, foliated shapes. One form merits particular attention: around a minute speck of silex there is formed a circular band of equally minute crystalline specks, and at a greater distance a second circular band concentric with the first, consisting of still smaller siliceous particles hardly visible in the microscope. By what atomic forces does this central crystal group its attendant crystals around it?

Mr. Stoney observed that Dr. Lloyd, at the Aberdeen Meeting, had shown that the light from these specimens of decomposed glass, exhibited by Sir D. Brewster, was elliptically polarized, and that therefore they must behave like uniaxal crystals.

Proceed. British Asso. for Adv. of Science.

On Fresh Water Wells near the Sea Coast.

From the London Athenæum, August, 1860.

Sir Emerson Tennent notices the fact of all the wells along shore which keep their water during the dry season, being below high-water mark, and that, to a small extent, they rise and fall with the tides; and he assumes that they owe their water to the sea, which loses its saline matter by percolation. Nothing, surely, is more utterly opposed to the first principles of Physics than the doctrine that salt held in chemical solution by water should be capable of being separated from it by the mechanical process of filtration. The phenomenon of tides in wells of moderate depth dug near the sea is of universal occurrence all along the Malabar coast, where the matter dug through is porous. It does not obtain in wells dug through trap. I have observed it hundreds of times at Bombay, and have often had occasion to describe it. The explanation is easy. The surface of the ground where the well is dug being always six or eight feet above high and twenty to twenty-six feet above low water, and being extremely spongy and porous down to where it comes in contact with the rock, or the blue clay bed which commonly lies over the rock, it gets charged full of the water during the rains. The superior length of column enables this to expel the sea water, a proceeding which must have been completed shortly after the emergence of the land from the sea; while the interstices in the porous soil are so minute as to prevent the two mingling. As the saltest sea water has only a specific gravity of 1·050, the fresh water ponded back from it requires only to be proportionally higher in level to create an equilibrium. With a greater head than this, it will push the wall of salt water before it, and flow off. Of all this, I have seen abundant examples at Bombay. It would occupy too much of your space to describe them. After six or eight months of rainless weather, when the discharge from the soil becomes feeble, the wells all become more or less brackish, and the apparent tide increases.

The *Edinburgh Review* states, that this theory of Sir E. Tennent, of the desalinization of sea water by filtration (as already said, a phenomenon opposed to one of the first laws of chemistry), explains the occurrence of fresh water on coral islands, and confutes the theory of Darwin, that this arises from rain, as rain falling on a substance already fully saturated with sea water would not be absorbed, but would flow off. Not a doubt of it. But coral islands are not only not saturated, but so much of them as is above the sea level three or four feet is highly porous and perfectly dry, and presents all the conditions for absorbing the whole of the rain that falls on them. They present to the rain this much head of water to push out the sea and expel it piston-wise so far as the coral bed descends—the sea itself forming the wall of the reservoir. A well dug deep into the coral to draw off the rain water, with which it is always nearly saturated up to low water mark, is sure to secure a supply. An illustration of the two not mixing together, if the pores of the soil, rock, or coral, be fine enough, may be obtained by making the experiment with capillary tubes.

For the Journal of the Franklin Institute.

The Expansion of Water NOT *an Anomaly in the Solidification of Liquids.*

In the article on expansion in that valuable work, the "New American Cyclopædia," reference is made to the dilatation of water when cooled down to near the freezing point, and to the beneficent effect of ice being thus caused to float, instead of sinking and choking up our rivers. The fact is mentioned, as it has long been mentioned in the books, as a remarkable exception to the general law of expansion of liquids in proportion as they are heated. Thus, Arnott, in his Physics, designates it as "a most extraordinary exception to the law of expansion by heat and contraction by cold," producing unspeakable benefits in nature, &c.

In Brande's Dictionary of Science it is said, "In general, all liquids expand and contract in proportion as they are heated and cooled, but to this law there is a remarkable and anomalous exception in regard to water."

According to the latest edition of the Penny Cyclopædia, "Water, like all other fluids and substances, expands by exposure to an increase of temperature, and, with a curious exception, the dilatation within certain limits is proportionate to the degree of heat to which it is subjected. It is, however, found that water, a few degrees above its freezing point, is more dense than exactly at it."

Jamieson, in his Dictionary of Mechanical Science, after stating that there are cases in which expansion is produced not by an increase but by a diminution of temperature, adds, "Water furnishes us with the most remarkable example of the kind." Dr. Draper, in his Text Book of Natural Philosophy, observes that solids and liquids do not expand with regularity, and that "there are special irregularities of which water is an example."

"Water is wisely ordained by God to be an exception to a very general law—it contracts till it is reduced to 42°, and then it expands till it freezes."—Guide to Scientific Knowledge, by Rev. Dr. Brewer of Trinity Hall, Cambridge. American ed., 1851.

In Kemp's Phases of Matter, Lon. 1855, we are told, "There is a most remarkable exception to this law of expansion in the case of water. Ice, as every one knows, swims upon water, and of course is lighter; * * that is to say, heat does not expand ice, but, on the contrary, contracts it. * * Whatever may be the cause, it is one of the most striking instances of design that can be witnessed in nature; and, were it not for it, the globe would scarcely be habitable by man. * * Did water obey the usual law in this respect, it would fall to the bottom as fast as formed."

"In freezing, there are some peculiar conditions connected with water. This fluid is at its maximum density at 40° of Fahrenheit; in cooling further, an apparent expansion takes place, and water cooler than this floats on the warmer fluid. This may, I think, be explained without that mysterious alteration of a fixed law which is sometimes had recourse to as a means of explanation. Water in cooling below this point, commences an arrangement of its particles preparatory to their passing into the solid form, different from that which previously obtained; the grouping of the molecules partakes of an angular rather than that of a spherical character, and hence in one direction they occupy more space," &c. Elementary Physics, by Robt. Hunt, Lon. 1855.

Considering the progress of science down to the close of the 18th century and its still further advances to the present day, it is singular that this alleged anomaly should have been so long taken for granted and stereotyped in works on Natural Philosophy. It is many years since it was questioned here by some members of the Mechanics Institute, because of its inconsistency with facts familiar to them. It was stated that it presented no "remarkable," "peculiar," "curious," "extraordinary," or "anomalous" exception, nor any exception at all to any law, but was in strict accordance with the one that governs the solidification of liquids—that if ice did not float, the fact would be an anomaly.

It was affirmed that, if our lakes and rivers were fluid metals, with their surfaces congealed in winter, the solid portions would swim as ice swims; and the proofs offered were that pigs of lead and tin float in liquid lead and tin, and that the like takes place with gold and silver, zinc, copper, and iron, as may be daily witnessed in the factories. The inference seemed to be that most, if not all, solids are less dense than are their liquids at certain temperatures, and those who doubted this were asked to name a liquid, either vegetable, animal, mineral, or metallic, in which portions solidified do not swim. Wax, pitch, rosin, fat, sugar, sulphur, and other substances, were named as affording no support of the common doctrine.

In passing into the solid state, the molecules of every liquid assume an arrangement more or less peculiar to it, and, as this must take

effect at some point of the decreasing temperature, it matters not where that point is as respects the common law of expansion. It no more affects that law than the journey of a traveler is affected by his stopping a moment to exchange a word with a friend. It neither affects his previous nor his subsequent progress—has, in fact, nothing to do with either.

The temperature of solidification of course differs in different substances, and so does the effect. It is the molecular arrangement that diversifies the crystalline structures, and consequently the properties of solids; that gives to each a "grain" and character peculiar to itself. In the soft metals, the crystalline texture would hardly be suspected, but it may be vividly brought out, even in lead, by crushing a mass just before solidification is completed. In iron foundries, the moment when the crystals are forming is often indicated by a rising of the metal in the gates of a mould.

It is known that the crystalline structure of metals is deranged by rolling, stamping, forging, wire-drawing, and other processes, but it is not commonly known that this effect is temporary, that they have power within themselves to recover their pristine formation. This we have noticed in drawn wire and pipes of block tin. When of pure metal, they are soft almost as lead, and yield to flexure as silently; but if laid aside a few years, they give out when bent the crackling noise by which bars of the metal are characterized.

A very interesting fact is mentioned by Scoresby. He found in the Arctic regions that water congeals there in an almost endless variety of geometrical figures, of which he enumerates five classes—the lamellar, the stelliform (which is most general, and occurs chiefly when the temperature is near 32°), the regular hexagon (which becomes thin and diminishes in size as the cold increases), aggregates of hexagons, which occur chiefly at low temperatures, and, lastly, combinations of hexagons with spines or radii.—Arctic Regions, vol. 1, p. 432.

That this change of structure occurs in the metals, is exceedingly probable, and that upon it the tempering of steel depends. We know that its different degrees of hardness arise from the different temperatures at which it is cooled—that is, from changes in the form of its crystals.

There is another point of resemblance between liquefied metals and water, which has not found a place in text books, though published by the Messrs. Appleton nearly twenty years ago, in Ewbank's Hydraulics, viz: capillary action.

*

New York, September 16, 1861.

Large Casting.—An enormous casting has been made in the foundry of Messrs. R. and G. Harris, of Rotherham. The total weight of the casting is 33 tons 10 cwt., and it is to form a bed for an immense hammer at the new works of Messrs. John Brown & Co., Sheffield. The metal was run from the cupolas in four minutes and a half.—*Builder.*

For the Journal of the Franklin Institute.

On the Erie Experiments on Steam Expansion by U. S. Naval Engineers. By SAMUEL MCELROY, C. E.

(Continued from page 237.)

When we examine another very important detail of method adopted in these experiments, viz: as to the measurement of coal consumed, we find it also inaccurate and unsatisfactory. This the printed explanation makes sufficiently plain; for when we are told that the notes for each experiment were commenced "with average fires" in the boilers, and that the fires were made at the close, "the same as at the beginning, *as nearly as could be estimated*," we understand at once that the Board only weighed what coal was used during the actual term of any trial, and *guessed* at the state of the boilers at its commencement and end. The precise quantity of coal which produced the recorded number of engine strokes was not *weighed*. For that essential particular, the Board asks us to accept its estimate, and this admission is in itself fatal to any claim of accuracy otherwise presented. Anybody can guess, but it belongs to experimenting engineers to measure and rigidly determine every detail, and especially so important a detail as the coal account, on which all the calculations of results depend. So long as it is easy to determine this account in experiments of this class, beyond any question, we are not prepared to accept any explanation or any apology for its omission. Those who have had frequent occasion to probe these processes, are not to be told that the absolute state of a boiler fire can be determined by its appearance, or its level, or the water level, or the steam gauge; for we know by the results of frequent analyses, that these features may be identical where there is a difference of fifty per cent. in absolute efficiency. Nor can we yield the professional rule in this case, which refuses to accept opinions and estimates where actual quantities are at issue. With the coal account in this state for each experiment, it is impossible for this Board to claim that it has *demonstrated* any thing. In entering voluntarily the field of estimate and conjecture, it submits to the "theoretical considerations" it claims to have overthrown.

Farther, in what way are we to reconcile this assumed process with the 1st and 2d experiments? At the close of the latter, the "average fires" represent a combustion of 3·79 lbs. per square foot of grate, while at its commencement, which also marked the close of the 1st experiment, the rate then represented is 6·28 lbs. per square foot. There can be but one conclusion in a case of this kind, and it is not very creditable to the discretion of this Board in matters of judgment and opinion. Not only is the accuracy of the coal account involved here, but the per centage of ashes, the boiler evaporation, the horse-power, and the resistances.

In experiment No. 5, at $\frac{7}{10}$ths cut-off, the coal account is credited with 18·53 per cent. of refuse, while No. 7, at $\frac{4}{10}$ths cut-off, is credited with but 6·99 per cent., which is not exceeded in any other experiment, none of them agreeing as to this per centage. In No. 5, a different

variety of coal was used from Nos. 1, 2, 4, and 7. But is it to be inferred from all these discrepancies that the coal varied so much in quality, or that the system of measurement was defective in accuracy? Under such circumstances, which is the most likely to be true? Two important conditions of the measurement were guessed at; the third only was determined.

We are disposed to concede to this report the merit of deep and abstruse argument. After looking over it very carefully, when it first came to hand, in some bewilderment as to what it really did assert, we endeavored to find some one place where a clear and unmistakeable conclusion was presented, and failed in discovering any thing more definite than the quotation with which we commenced this paper. But in those brief sentences, strange contradictions occur. They begin by admitting a gain with two-fifths cut-off; they assert a loss at one-quarter, a loss which we might assume to be referred to full travel, if one-sixth was not immediately presented as such measure. And yet, a page or two back, we are told that if "the proper corrections could be made for the difference of cylinder temperature due to the different measures of expansion, it would doubtless be found that the economical result obtained when cutting off at seven-tenths, is not exceeded when cutting off at any less fraction."

It is not an easy matter, therefore, to pass from general questions of principle and method to a discussion of absolute results, so many different ones having been obtained. The general argument of the report is against the use of expansion at all. The summary of all the allowances, assumptions, and equivalents in table No. 2, ranges the "economic result in net horse-power" with $\frac{4}{45}$, $\frac{1}{6}$, $\frac{11}{12}$, $\frac{1}{4}$, $\frac{3}{10}$, $\frac{7}{10}$, $\frac{4}{9}$ths cut-off in order of precedence, while the "economic power" in "pounds of steam per horse-power per hour," ranks them $\frac{4}{9}$, $\frac{7}{10}$, $\frac{3}{10}$, $\frac{1}{4}$, $\frac{11}{12}$, $\frac{1}{6}$, $\frac{4}{45}$ths cut-off, which exactly reverses the order. Precisely what conclusion the Board really reached does not appear in the report; while its argument rejects expansion, its tables confirm it in part, in instances which contradict its explicit assertions.

In table No. 1, the order of results for the "pounds of combustible" per horse-power, is $\frac{1}{4}$, $\frac{4}{9}$, $\frac{7}{10}$, $\frac{3}{10}$, $\frac{1}{6}$, $\frac{11}{12}$, $\frac{4}{45}$ths cut-off.

Passing from this troublesome triangular tabular duel, we may take up the assertion, that "the economy of cutting-off at one-sixth or four-forty-fifths is considerable less than with steam used absolutely without expansion," for the purpose of tracing the argument of the report.

Assuming that the "data" given in our synopsis of table No. 1 were rigidly and accurately obtained, we cannot compare the results of one-sixth cut-off with full steam travel, because the latter was not tested; but taking eleven-twelfths as an approximation, the horse-power per pound of coal is in favor of the former as 5·53 to 4·58, although there is a loss of ·9 inch in vacuum.

In this case, the horse-power is determined by the mean pressure of the indicator cards, the revolutions, and the coal account in the usual way.

But the Board decides the propriety of making certain corrections to these "data."

The per centum of refuse in the coal account varies in each trial. It is as low as 5·75 and as high as 18·53. For one-sixth cut-off it is 5·75, and for eleven-twelfths it is 6·83. The latter is therefore credited with the difference. Having already expressed our opinion of the special accuracy of experiment No. 2, which cuts off at $\frac{1}{6}$th, we need not reiterate obvious objections. It is enough to suggest that if, in experiments as delicately conducted as these, the same boiler work could not be realized in similar kinds of coal, a principle under investigation ought not to be charged with the difference, and also, that it is not perfectly clear that the difference between ashes in weight and that of the coal producing them, represents the absolute evaporative value of the coal. The manner in which the coal is burnt has something to do with that point.

There is a difference of vacuum against No. 2 which we do not find credited.

Experiment No. 7 ($\frac{11}{12}$ths) shows an average back-pressure in the cylinder of 4·2 lbs., while No. 2 shows but 2·8 lbs. In all these trials, it appears that expansion reduces back-pressure by a descending series, except a change in No. 7. But, without accepting this *experimentum crucis* of the condensation argument, the Board decides that back-pressure must be assumed at a common standard, which it accordingly takes at 2·7 lbs. from No. 3. Consequently, the mean pressure of No. 2, which is 13·6 lbs., is to be charged with the standard of 2·7, while that of No. 6, at 29·8 lbs., is credited with the difference between 4·2 and 2·7. This varies the relative horse-power, and is highly creditable to the treatment of the questions at issue.

The Board then enters into a long argument to show that the "net horse-power applied to the water by the paddles" is the only correct basis of calculation of work, rejecting the work which is done by the piston. This involves a difficulty, slight, however, to ingenious men. After the trials are over, they spin the wheels around at various speeds, and determine by indicator cards the exact "pressure due to the friction and resistance" of the organs of the engine, which they establish at the common standard, for all loads and all speeds, of 2·1 lbs. per square inch. The mean pressure of No. 7 bears this charge much more easily than that of No. 2.

In this case, the expansion is charged with the interesting fact that the friction of an engine, and especially an engine paddling water, is constant at all speeds, and under all loads. If the velocity varies in the proportion of 11·17 to 20·61, and the load varies in the proportion of 13·6 to 29·8, the effort is precisely 2·1 lbs. per square inch in each case. It is 15·4 per cent. in one, and 7 in the other. Morin, Weisbach, and others, have therefore been convicted of many "fallacious theories" on this matter of frictional and fluid resistance. Their results are entirely reversed.

And yet, granting all these equivalents, when we reach the last point of calculation, we find that the experimental result in net horse-

power for No. 6 is 1000, and for No. 2 is 1065. It is not true, then, that it is better to cut off at $\frac{1}{12}$ths than at $\frac{1}{4}$th, and, by consequence, it is not true that it is still better to follow full stroke. The summary of the report falls to the ground: *vox et præterea nihil.*

But the economy of full steam, we are told, is comprehended in the use of smaller engines, which are to save first cost, space and repairs. In what respect, except as to the bore of the cylinder, is the *Michigan's* engine to be reduced? The wheels, shaft, cranks, and connexions, condenser, air-pump, &c., at one end must retain their present strength and size, and at the other end the boilers must be as large and require as large coal bunkers. In reducing the bore of the cylinder, it would be improper to reduce the side-pipes and valve-chests, and the piston-stroke should not be shortened. Nothing, then, can be saved, except a few pounds of cast iron, a few bolts, and a little wrought iron and rubber. The modified engine will go to sea like one of the fashionable belles denounced in medical journals: the head, arms, and lower limbs fully developed, but the seat of vital action laced and compressed beyond all reason and contrary to natural health. If the Board holds that the friction of an engine is independent of its load, it may also hold that "wear and tear" is equally independent; otherwise the working parts save nothing in repairs or liability to fracture.

Again, we are informed in the report, that the pressure defined by the law of Mariotte, as derived from "abstract considerations," and illustrated by indicator cards, is "so specious, and apparently so conclusive (as a promise of economy in expansion), that up to within the last one or two years, the assumption passed unchallenged by the engineering profession;" but the Erie experiments claim to have overthrown this specious assumption.

We have analyzed these experiments sufficiently to show that, however improper their *modus operandi*, their results do not prove any loss in power. Let us now examine the theory of a loss which is asserted and not demonstrated.

It is asserted that, in the case of full steam travel, as the piston gradually uncovers the surfaces previously exposed to the exhaust, a condensation takes place; so that, at the end of the stroke, the cylinder surface is covered with a film of water at exactly the boiling-point, due to the pressure of its steam charge. When the exhaust valve now opens, this water evaporates, and the value of its heat is lost in the condenser.

When the cut-off is used, the condensation goes on in the same way, except that the film of water condensed before the valve closes commences to evaporate as the pressure falls, and re-evaporation takes place on the surface of the cylinder throughout the stroke, instead of after the return stroke, cooling down the surface during the whole time of expansion movement. This loss is said to be of a very serious character, and, like that due to full travel, must be made up at every new stroke.

This Board is twice mistaken. First, in assuming that engineers

have depended on realizing the absolute results of Mariotte's law, without the modifications due to conditions of practice; and, second, in assuming the merit of discovering this process of condensation.

The losses due to imperfect combustion and evaporation, foaming, condensation in steam passages, leakage of valves and joints, and back-pressure, certainly have been fully admitted, and are always anticipated. And precisely as far as these may, in practice, modify the results of an absolute law, our confidence in the law itself need not be affected. Imperfections in application, instead of inclining us to this monstrous argument, which would dispense with expansion because some of its benefits are vitiated, should only prompt us to the construction of more perfect machinery, by which the law itself may have a better development. No single portion of this report, no result attained, disproves the correctness of the law, and its whole argument, rightly understood, vindicates expansion against imperfect mechanism, imperfect management, and prejudiced experts.

As long ago as 1782, the master mind of the steam engine, in proposing to cut off at one-quarter, was discussing the effects of this principle of condensation. Since that time, all the way down, engineers have taken ordinary and extraordinary precautions against it. They have built fires under their cylinders, they have placed cylinders within cylinders, they have built around them brick-work houses, they have exhausted the varieties of non-conducting materials, in all kinds of felting and jacketing. There is not any thing new, then, in this discovery of condensation, or the apparently neglected operation of external radiation.

Nor is it true that experiments on the assumed losses by condensation are at all novel. The author of the "Precedents" only provoked a smile when he congratulated himself as the first to compare the *tank* with the *indicator*. The idea is not at all patentable.

Nor is it any thing of a novelty that comparisons between the tank and indicator should, on account of imperceptible boiler waste, foaming, steam-pipe and cylinder condensation, valve leakage, &c., show a per centage of difference depending on the comparative protections used against these losses. Nobody disputes it. Everybody anticipates it.

So fully, in fact, are engineers advised on this point, that when any experiment is presented to them, no matter by whom conducted, which claims to have found but 2·91 per cent. loss between the tank and indicator, they respectfully deny its accuracy. It is impossible to avoid a greater loss in the boiler itself, and between the boiler and steam-chest, and at the valves, as well as in the cylinder. Take the case actually presented. The boiler pressure for experiment No. 6 is 36 lbs. and at the cylinder valve we have 34·9, or a little over 3 per cent. in that item alone. In No. 7, the boiler pressure is 36·9 and the pressure at the valve 34·8 lbs., or 5·7 per cent. less, in this respect alone. No credence whatever, then, can be given to the calculation, which sums up all the losses in $\frac{1}{14}$ths steam travel at 2·91 per cent., finding them for $\frac{4}{15}$ths, 37 per cent. The indicator cards given for experiment

No. 6, show a final pressure of 29·3 lbs., whereas, with an initial pressure of 34·8, it should not have been less than 31·9. Here is a loss of 2·6 lbs. to be accounted for, or about 8 per cent., making a total for but two items of all those in force of 11 per cent., which the indicator cannot show. When we turn to the expansion card of No. 2 and No. 7, on the other hand, we find that the final pressure in the first case is 7·8 lbs., when it should not be over 5·71, being 2·09 lbs., or 37 per cent. in excess; in the second case it is 5·9 lbs. instead of 3·02, being 2·88 lbs., or 95 per cent. in excess. This excess the indicator has accounted for, as well as the tank, but the book-keeping of the Erie Board brings expansion still in debt.

Granting, for the moment, the correctness of the theory of condensation we have quoted, as an argument against expansion, when we come to compare it with the losses due to any condition of operation, what is its practical amount? Expanding or not, at every stroke the cylinder surface is exposed to the action of the exhaust, which must be much more formidable than the action of the steam charge, no matter what its conditions. Whatever this loss may be, is it not true that its effect, after the engine has attained uniform action and after the main valve closes, is confined entirely to the particular charge of steam enclosed by the valve in the cylinder; and, inasmuch as *pressure*, *temperature*, and *volume*, are rigid measures, one of the other, how can it be denied that the indicator is a correct index of all such effects? As the indicator card does not in reality measure the operation of any given stroke on each side of the piston, but combines the steam travel of one stroke with the exhaust travel of the succeeding one, it is also a measure in any special steam travel of the effects of its precursor; and, until it can be proved that the volume and temperature of the steam charge can be changed without affecting its pressure, pressure must be taken as a direct index of each. We have looked in vain through this report for any positive denial of this principle. At the very close of the elaborate discussion of losses by condensation, it is stated that, "if there be any portion of the stroke during which the steam loses the form of vapor, a dynamic effect measured by that portion and the *wanting pressure*, is lost." It is beyond reason, then, to claim that the indicator will not measure any such "wanting pressure."

This theory of special loss by condensation, in expanding, must be tested by its evidences. Various experiments have been made at different times and by different authorities, with different results. Pambour, on one side, determines a slight loss, while Pole invariably discovers a gain. We have notes of careful experiments on an engine working generally under one-fifteenth cut-off, where the sum total of all losses is 23·4 per cent. In other cases we have found it 16 per cent. As a matter of testimony by experiment, then, the "data" of the Board must face numerous results by no means "fallacious," or "specious," or "purely theoretical."

To return to the argument of the indicator cards:—In experiment No. 6, there is a loss in final pressure of 8 per cent., and in back-pressure, as referred to *mean pressure*, there is a loss of 14 per cent.,

and of 12 per cent. in *initial pressure*. In experiment No. 2, there is an excess in final pressure of 37 per cent. beyond that due to the initial steam and expansion, while the back-pressure is 20·6 per cent. of the mean pressure, and 8·2 per cent. of the initial. In experiment No. 7, the excess of final pressure is 95 per cent., the back-pressure being 42 per cent. of the mean, and 10·9 per cent. of the initial. Certainly there is no argument in such a state of facts as to losses *in the cylinder* by expansion, but there is a most fatal argument against the parade of accuracy, and perfect machinery, and valves which could not possibly be supposed to *leak*.

The experiments, as to back-pressure, confirm a point of simple demonstration, viz: that the reduction of steam volume per stroke involves a reduction of back-pressure, as referred to initial pressure, and that this item, in comparing similar volumes doing the same work, is not increased by expansion. All the subtle deductions of the report on this point are incorrect, being disproved by its own results. As to economy of work, it appears that there is an absolute excess of pressure at the highest rate of expansion, and nearly double the final pressure due to the Mariotte law, which is a waste of power and steam to an enormous extent, and is chargeable to leaky valves, being an item of credit to the expansion account. When we remember that in boiler priming the results in waste are formidable against full steam travel; that this matter of condensation as applied to expansion *per se* and compared with other palpable losses, can have but little effect; that the whole course of these experiments tends to prevent the true illustration of economy in expansion, and does not assert the opposite in result; we may well be content to rest the examination of results at this point. If the Erie Board, in expanding $\frac{4}{5}$ths, burned over 6 lbs. of coal per horse-power per hour, we may readily accept the testimony of those engines which, at the same expansion, burn 2 lbs. per horse-power.

A certain mechanical principle underlies and controls the whole question of expansion, although its connexion is not commonly recognised. A principle which belongs to the primitive formations of all engineering theory and is indissolubly united to the very elements of motion. Our allusion to it involves a slight historical discussion.

In the abstract of this report given in the April number of this journal, Watt is credited with the first application of expansion as suggested to him by the announcement of Mariotte's law. The writer is in error in two respects; first, by the fact that Hornblower preceded Watt six years in the application of expansion as a source of economy, and second, that Watt's original application of the cut-off was made in view of the great principle to which we allude, viz: the effect of the *mass of an engine in motion*. Nor is the speculation as to Watt's unpublished experiments on expansion leading him to adopt a steam travel of three-quarters probable, as he made the mistake of the Erie Board and vitiated the results within his reach by using too low pressure. He proposed in 1782 to cut off at one-quarter. Trevithick in 1806 apprehended the question of economy much more

fully, using steam at 40 lbs., and proposing to build an engine to cut off at less than one-sixth. And since that time, the whole Cornish school, instead of confining itself to this standard, has carried the grade of expansion in some cases to one-twentieth, not for purposes of experiment, but for regular duty. It is a very great mistake to suppose or to assert that, "until quite recently, it was the exception, and not the rule, to find new engines cutting off at less than one-half."

But without pausing here to sustain a very simple matter of record, we refer again to the fact, that when the genius of Watt superseded the *atmospheric* engine and used steam as a driving power, it also comprehended an inevitable law of motion, which demanded the application of the cut-off as a mechanical necessity, in advance of any idea of economy. We take an impregnable position, then, based on absolute principles, when we assert that the cut-off is an appurtenance which bears to every engine in full motion a relationship entirely independent of any question of economy, although this is a natural sequence, and that the idea of assuming full steam travel as a basis of comparative mechanical action is a misapprehension of engine duty.

The argument on this point is sufficiently clear in reference to all bodies in motion which have weight. To overcome the inertia of an engine, a certain surplus pressure must be applied to the piston, which corresponds with initial pressure, and is exceeded at no after point of the stroke. The mass being thus put in motion by charging it with surplus power, it is a mechanical absurdity to continue the initial pressure any farther than will suffice to complete the stroke by virtue of the surplus power imparted at the commencement. In the general application of this law, there is no distinction between single-acting and fly-wheel engines; mass in motion characterizes both.

It is an absolute necessity, then, in every engine, that the power necessary to complete its stroke properly, must be imparted to it in excess at an early period of such stroke; and inasmuch as the whole experience of the steam engine in practice abundantly confirms the theoretical conclusion that this surplus power may be exerted at a very early point of motion, this disposes of the expansion question, not only as to mechanical effect, but as to economy. For all the fine drawn arguments on condensation and re-condensation are of very little consequence to the mass which is by this time distributing its excess of power.

Viewed in this light, the doctrine of expansion divests itself of all incumbrances. We come back again to the principle of maximum useful effect. There is a given velocity to be imparted to a given load at the start. If a steam travel of four feet under ten pounds pressure will do it, who is to assert that a travel of one foot under forty pounds pressure will not do it equally well, better in fact, and much more cheaply? No experimental philosophy can prevail against a plain mechanical law like this, and certainly no such experiments as those we have here discussed. On the contrary, the most extensive, severe,

laborious research, by the first men of the age, has brought out this law "seven times refined" for the benefit of the world. So long as we know that the maximum velocity of motion can be imparted to an engine before it reaches the half-stroke, we decide the fallacy of any argument which prescribes any later point of cut-off; and we also decide that the only limit to economy of steam by expansion, is to be determined by the practicable conditions of such initial motion, and the practicable perfection of construction.

Paper from Wood.

Many years ago, a M. Watt succeeded experimentally in manufacturing paper of fine quality from woody fibre. But the large quantity of concentrated alkalies which he was forced to use in a highly heated state, prevented the practical introduction of his process. It appears that a French lady, whose name is not given in the *Cosmos* to which we are indebted for the following account, has succeeded in avoiding the difficulty by the use of a peculiar cutter, by which the wood is reduced to a species of *lint* before it is subjected to the reagents. This cutter consists of a series of parallel wheels, set close together on an axis, and armed with fine points, which penetrate the surface to a small depth (not more than one-hundredth of an inch), and are followed by a sort of plane which ploughs off the surface thus minutely divided. The material is then made into a pulp by the action of acids and alkalies, and bleached by chlorine. The paper thus made is said to be equal or even superior to linen paper, and to be fitted for use for fine impressions of engravings in place of China or India paper, while its cost is less than one-hundredth of that material.—*Cosmos.*

On the Nature of the Deep-Sea Bed, and the Presence of Animal Life at Vast Depths in the Ocean. By Dr. G. C. Wallich.

From the Lond. Engineer, No. 279.

(Continued from page 241.)

The *Foraminifera* are the organisms to which reference has been made as performing so very important a part in the formation of certain strata on the earth's crust. They occur abundantly in all existing seas. They are to be met with in a fossil state, not only in chalk, but in almost all marine sedimentary strata; as, for instance, in the hard limestones and marbles. The recent Foraminifera may therefore be looked upon as the oldest living representatives of any known class of organisms.

In the mud, or "ooze" as it has been termed, which is brought up from great depths in many parts of the open sea, immense assemblages of Foraminifera are to be met with, chiefly belonging to one species, however. In the absence of examinations conducted immediately on their being brought up to the surface by the sounding ma-

chine, it is not surprising that the question as to their occurrence in a living, or only in a dead state, should have remained undecided. Most of the authorities who have written on the subject being of opinion that they do not live at great depths, but that their shells and remains have drifted to the positions in which they were found from shallower waters, or have subsided from the upper strata of the ocean. Professor Huxley was one of the very few who leant to the more correct opinion; he having declared that, although far from regarding it as proved that the Globigerina (the species referred to) live at these depths, the balance of probabilities seemed to him to incline in that direction. Other writers have offered surmises on the subject; but these, in the absence of any thing like substantial proofs, were, of course, only estimated at what they were worth.

The difficulty is how to determine the point conclusively. For it seems legitimate to infer that, if these organisms are specially adapted to exist under conditions differing so widely from those present at or near the surface, the very circumstance of removing them from one set of conditions to the other, would inevitably destroy their vitality, and, perhaps, their normal structure, before it could become practicable to subject them to microscopic analysis. Nor is the difficulty an imaginary one. For, taking into consideration the entirely altered circumstances in which these creatures must find themselves placed when brought to the surface, locomotion, or even the protrusion of their filamentary appendages, could hardly be expected. The mere existence of the fleshy parts within their shells, and that too in an apparently recent condition, affords no proof, inasmuch as the great quantity of saline matter present in sea water, and especially at great depths, would, of itself alone, account for their perfect state of preservation.

During the recent survey of the North Atlantic, I found that, in certain localities where the Globigerina deposit was of the purest kind and in the greatest plenty, the specimens from the immediate surface stratum of the sea bed alone retained their normal appearance, both as regards the perfect state of the sarcodic contents of the shells, and the presence of the pseudopodia. The latter organs were never seen by me in an extended condition; but in the specimens alluded to, and in those only, occurred as minute bosses, resembling in shape the rounded rivet-heads on boilers, closely appressed to the external surface of the shell; whereas, in specimens from the sub-stratum, the color was much duskier, and these bosses were absent. And, further, in these pure deposits the shells were to be found in every gradation, from the single chamber, of microscopic minuteness, hyaline transparency, and extreme thinness, to the dense Zeolite-like structure of the many-chambered mature shells, which are large enough to be readily distinguished by the naked eye. These facts, when taken in conjunction with the entire absence of the varied remains of other organized structures found in localities where the Globigerinæ are only scantily represented, afford, as I conceive, all but the direct proof, which can only be arrived at on witnessing locomotion, or the

protrusion and retraction of the pseudopodia of the organisms in question.

Most fortunately, as it happened, this collateral evidence was rendered doubly conclusive by other proofs of a most unexpected and interesting kind. Before entering on these, I may state that the substratum, spoken of as differing in aspect from the immediate surface-layer, is, nevertheless, identical in composition; the difference in color arising simply from decay. It contains no living Foraminifera; for the minute particles of matter becoming gradually condensed and aggregated together by molecular affinity, and the enormous superincumbent pressure exerting itself only in one direction, that is, vertically, its permeability by fluids is thus completely destroyed, and it is compacted into a dense mass of far too unyielding a nature to admit of its being traversed by living creatures of any kind. As the Foraminifera die off, their shells and decaying contents, together with the minute particles of amorphous matter associated with them, go to build up the calcareous strata of the earth's crust. I would mention that, in order to determine whether the *Globigerinæ* live as free floating forms in the mid-strata of water, I attached a small open-mouthed bag, at about 200 fathoms from the extreme end of the sounding line, in a locality where the species was most abundant in the deposit, and brought it up through nearly 5000 ft. of water, without securing a single shell.

But by far the most important and interesting discovery remains to be noticed, viz: the detection of a high order of radiate animal in a living state, at a depth of a mile and a half below the surface of the sea.

When we take into consideration the low position of the rhizopod in the scale of being, and the obvious probability, pointed out by Professor Huxley, that a class of creatures proved to extend so far back in time—that is, in a fossil state—must be able to maintain existence under extraordinary and variable conditions as regards light, temperature, and pressure, the sentiment engendered is rather one of wonder that their vitality at great depths should have been so long and so stoutly maintained, than that it should now be so fully proved. But few persons were bold enough to suspect that creatures of a far higher type, viz: Radiata, could exist under similar conditions; and I freely admit that nothing short of the most incontrovertible proof ought to be accepted in support of such a view. Fortunately, I am in a position to afford that proof.

In sounding midway, in the direct line between Cape Farewell, the southern point of Greenland, and the north-west coast of Ireland, in lat. 59 deg. 27 min. N., and long. 26 deg. 41 min. W., the depth being 1260 fathoms (or 2520 yards), whilst the sounding apparatus itself brought up a considerable quantity of minute granular particles, looking like a fine oolite, but which was, in reality, a nearly perfectly pure Globigerina deposit, thirteen star-fishes, from 2 ins. to 5 ins. in diameter from tip to tip of rays, belonging to a genus plentifully represented on our own coasts, came up adhering to the extreme 50

fathoms of sounding line. These *Ophiocomæ* were not only alive on being brought up out of the water, but some of them continued for fully a quarter of an hour to move about their long spinous arms. To render intelligible the significancy of the entire circumstances, I must mention that, in order to insure accuracy, it is always necessary, when sounding in deep water, to ascertain the depth by one sort of apparatus, and to bring up the sample of bottom by another. In the present case, the ascertained depth was 1260 fathoms, and 50 fathoms was accordingly "paid out" in the second operation of bringing up bottom, in order to make sure that the more complicated and unmanageable apparatus required for this purpose fairly rested on the bottom.

Now, supposing it possible that these star-fishes were drifting about in some intermediate stratum of water, between the bottom and surface, it is evident that they would have attached themselves indiscriminately to any portion of the entire 1260 fathoms of line; unless, indeed, they chanced to have been directing their course in a closely compacted column, which was traversed by the last extra 50 fathoms of line at the precise moment of their crossing it. Whether it be possible that they were drifting in such a column, or floating on a bed of seaweed or other substance, is immaterial, inasmuch as they could only have attached themselves as they did to the portion of line referred to under this one condition. But the very act of attachment would, I maintain, be impossible in the case of creatures whose movements are so sluggish, when the object which they had to grasp was moving upwards at the rate of two miles per hour (as it does when hauled up by the steam engine), and without a moment's intermission. But even assuming it to be possible that they had drifted to the position in which they were captured, from distant and less profound depths, the fact of their vitality and vigorously healthy condition would be scarcely less extraordinary; for the distance from the nearest point of land, which is a rock off Iceland, is 250 miles; whilst the next nearest land, Greenland, is distant no less than 500 miles. But it must be obvious to every one who is at all conversant with the structure of the *Ophiocomæ* and Echinoderms generally, that they are essentially creeping and crawling creatures, and of far too great specific gravity to float at all under any circumstances.

Taking into consideration, then, the circumstances under which these *Ophiocomæ* were taken, the extreme improbability of their having drifted to the locality in which they were found from distant and shallower waters, and, lastly, the peculiarities of structure which render them wholly unfit to float or swim for even a brief period, we should have been fully warranted, I think, in believing that they existed in a living state at the bottom. In order to obtain some clue to the solu tion of the question, I very carefully dissected and analyzed the contents of the digestive cavity of a specimen, immediately on its being brought up; and was most amply repaid by the detection of numerous *Globigerinæ* in every stage of comminution, and with the contained sarcodic matter in greater or less quantity. Whilst, therefore, the detection of these organisms in the digestive cavities of the *Ophio-*

comæ, afforded a most conclusive proof that the Foraminifera were living on the sea bed at the profound depth from which they were obtained, the fact of the star-fishes being captured with the fresh remains of the Foraminifera in their digestive cavities, proves that their normal habitation is at the same great depth, inasmuch as it has been sufficiently established that the *Globigerinæ* are present only at the bottom. I may mention that, within the past few days, in examining a sample of the *Globigerina* deposit, brought up by a previous sounding on the same spot, I detected some Echinoderm spines, which at once struck me as being identical with those on the *Ophiocomæ;* and that, on comparison, my surmise proved to be quite correct: a further and very striking proof of the vitality of the *Ophiocomæ* at the bottom being thus afforded.

For the Journal of the Franklin Institute.

Strength of Cast Iron and Timber Pillars: A series of Tables showing the Breaking Weight of Cast Iron, Dantzic Oak, and Red Deal Pillars. By Wm. Bryson, Civ. Eng.

D = diameter or side of the square of solid pillar in inches.
D = external diameter of hollow pillar in inches.
d = internal diameter of hollow pillar in inches.
L = length or height of the pillar in feet.
W = breaking weight of long pillars in tons.
Y = breaking weight of shorter pillars in tons.

Mr. Hodgkinson gives the following formulæ for the breaking weight of Dantzic Oak and Red Deal pillars, when the length of the pillars exceeds 30 diameters, both ends being flat and firmly fixed:—

Solid square pillar of Dantzic Oak (dry), $W = 10{\cdot}95\frac{D^4}{L^2}$.

Solid square pillar of Red Deal (dry), $W = 7{\cdot}81\frac{D^4}{L^2}$.

For shorter pillars, . . $Y = \frac{W c}{W + \frac{3}{4}c}$.

W = the weight calculated from either of the preceding formulæ.
c = the crushing force of the material.
Y = the breaking weight in tons.

The following formulæ are applicable for the breaking weight of solid cylindrical pillars of Dantzic Oak and Red Deal, both ends being flat and firmly fixed, and the length of the pillars exceeding 30 diameters and upwards:—

Dantzic Oak, $W = 6{\cdot}71\frac{D^4}{L^2}$. Red Deal, $W = 4{\cdot}79\frac{D^4}{L^2}$.

" $W = 4{\cdot}81\frac{D^{3.55}}{L^{1.7}}$. " $W = 3{\cdot}47\frac{D^{3.55}}{L^{1.7}}$.

Solid Square Pillars of Dantzic Oak, Both Ends being Flat and Firmly Fixed.

Length or height of Pillar in feet.	Number of diameters contained in the length or height.	Side of the Pillar in inches.	Calculated breaking weight in tons from formula, $w = 10{\cdot}95 \frac{D^4}{L^2}$.	Calculated breaking weight in tons from formulæ, $w = 10{\cdot}95 \frac{D^4}{L^2}$, $Y = \frac{w\,c}{w + \frac{3}{4}c}$.
5	30	2		5·56
6	36	"		4·40
7	42	"	3·57	
8	48	"	2·73	
9	54	"	2·16	
10	60	"	1·75	
11	66	"	1·44	
12	72	"	1·21	
5	20	3		18·74
6	24	"		15·96
7	28	"		13·58
8	32	"		11·58
9	36	"		9·93
10	40	"		8·55
11	44	"	7·33	
12	48	"	6·15	
5	15	4		40·27
6	18	"		36·03
7	21	"		32·02
8	24	"		28·37
9	27	"		25·09
10	30	"		22·28
11	33	"		19 80
12	36	"		17·64
13	39	"		15·78
14	42	"		14·17
15	45	"	12·45	
16	48	"	10·95	
5	12	5		69·76
6	14·4	"		64·35
7	16·8	"		58 94
8	19·2	"		53 74
9	21·6	"		48·85
10	24	"		44 33
11	26·4	"		40 23
12	28·8	"		36·56
13	31·2	"		33·20
14	33·6	"		30 23
15	36	"		27 58
16	38·4	"		25·22
17	40·8	"		23 11
18	43·2	"	21·12	
19	45·6	"	18·95	
20	48	"	17·10	

Solid Square Pillars of Red Deal, Both Ends being Flat and Firmly Fixed.

Length or height of Pillar in feet.	Number of diameters contained in the length or height.	Side of the Pillar in inches.	Calculated breaking weight in tons from formula, $w = 7{\cdot}81 \frac{D^4}{L^2}$.	Calculated breaking weight in tons from formulæ, $w = 7{\cdot}81 \frac{D^4}{L^2}$, $Y = \frac{w\,c}{w + \frac{2}{3}\,c}$.
5	30	2		4·24
6	36	"		3·32
7	42	"	2·55	
8	48	"	1·93	
9	54	"	1·54	
10	60	"	1·24	
11	66	"	1·03	
12	72	"	0·86	
5	20	3		14·83
6	24	"		12·42
7	28	"		10·43
8	32	"		8·79
9	36	"		7·47
10	40	"	6·32	
11	44	"	5·22	
12	48	"	4·39	
5	15	4		32·64
6	18	"		28·76
7	21	"		25·22
8	24	"		22·09
9	27	"		19·36
10	30	"		17·01
11	33	"		15 00
12	36	"		13·28
13	39	"		11·81
14	42	"	10·20	
15	45	"	8·88	
16	48	"	7·81	
5	12	5		57·31
6	14·4	"		52·25
7	16·8	"		47·31
8	19·2	"		42·66
9	21·6	"		38·38
10	24	"		34·51
11	26·4	"		31·06
12	28·8	"		27·98
13	31 2	"		25·27
14	33 6	"		22·87
15	36	"		20·75
16	38·4	"		18·88
17	40·8	"	16·89	
18	43·2	"	15·06	
19	45·6	"	13·52	
20	48	"	12·20	

Solid Cylindrical Pillars of Dantzic Oak, Both Ends being Flat and Firmly Fixed.

Length or height of Pillar in feet.	Number of diameters contained in the length or height.	Diameter in inches.	Calculated breaking weight in tons from formula, $W = 6{\cdot}71 \frac{D^4}{L^2}$.	Calculated breaking weight in tons from formulæ, $W = 6{\cdot}71 \frac{D^4}{L^2}$, $Y = \frac{W C}{W + \frac{3}{4} C}$.
5	30	2		3·74
6	36	"		2·91
7	42	"	2·19	
8	48	"	1·67	
9	54	"	1·32	
10	60	"	1·07	
11	66	"	0·88	
12	72	"	0·74	
5	20	3		13·24
6	24	"		11·02
7	28	"		9·20
8	32	"		7·73
9	36	"		6·54
10	40	"	5·43	
11	44	"	4·49	
12	48	"	3·77	
5	15	4		29·42
6	18	"		25·78
7	21	"		22·49
8	24	"		19·60
9	27	"		17·11
10	30	"		14·98
11	33	"		13·17
12	36	"		11·63
13	39	"	10·16	
14	42	"	8·76	
15	45	"	7·63	
16	48	"	6·71	
5	12	5		51·99
6	14·4	"		47·16
7	16·8	"		42·50
8	19·2	"		38·15
9	21·6	"		34·21
10	24	"		30·63
11	26·4	"		27·46
12	28·8	"		24·68
13	31·2	"		22·22
14	33·6	"		20·07
15	36	"		18·17
16	38·4	"	16·38	
17	40·8	"	14·51	
18	43·2	"	12·94	
19	45·6	"	11·61	
20	48	"	10·48	

Solid Cylindrical Pillars of Red Deal, Both Ends being Flat and Firmly Fixed.

Length or height of Pillar in feet.	Number of diameters contained in the length or height.	Diameter in inches.	Calculated breaking weight in tons from formula, $w = 4{\cdot}79\frac{D^4}{L^2}$.	Calculated breaking weight in tons from formulæ, $w = 4{\cdot}79\frac{D^4}{L^2}$, $r = \frac{wc}{w + \frac{1}{2}c}$.
5	30	2		2·83
6	36	"	2·12	
7	42	"	1·56	
8	48	"	1·19	
9	54	"	0·94	
10	60	"	0·76	
11	66	"	0·63	
12	72	"	0·53	
5	20	3		10·36
6	24	"		8·49
7	28	"		6·99
8	32	"		5·81
9	36	"	4·79	
10	40	"	3·87	
11	44	"	3·20	
12	48	"	2·69	
5	15	4		23·60
6	18	"		20·30
7	21	"		17·53
8	24	"		15·10
9	27	"		13 04
10	30	"		11·33
11	33	"		9·89
12	36	"	8·51	
13	39	"	7·25	
14	42	"	6·25	
15	45	"	5·44	
16	48	"	4·79	
5	12	5		42·39
6	14·4	"		37·95
7	16·8	"		33·78
8	19·2	"		29·97
9	21·6	"		26·57
10	24	"		23·59
11	26·4	"		20·99
12	28·8	"		18·72
13	31·2	"		16·75
14	33·6	"		15·05
15	36	"	13·30	
16	38·4	"	11·69	
17	40·8	"	10·35	
18	43·2	"	9·23	
19	45·6	"	8·29	
20	48	"	7·48	

In the following table, Mr. Hodgkinson gives the relative strength of pillars of different British irons as obtained from 22 solid cylindrical pillars of cast iron, 10 feet long and 2½ inches diameter, cast out of 11 kinds of iron (9 simple irons, and 2 mixtures). The pillars were all from the same model, and were cast vertically in dry sand, and turned flat at the ends, two being cast from the same kind of iron in each case. The simple unmixed irons tried were as below, and all of No. 1 iron.

Simple Irons.		Mean breaking weight in tons.
Old Park iron,	Stourbridge, .	29·50
Derwent "	Durham, .	28·03
Portland "	Tovine, Scotland, .	27·30
Calder "	Lanarkshire, .	27·09
Level "	Staffordshire, .	24·67
Coltness "	Edinburgh, .	23·52
Carron "	Stirlingshire, .	23·52
Blaenavon "	South Wales,	22·05
Old Hill "	Staffordshire, .	20·05

The pillars formed of mixed irons were found to be weaker than the three strongest of the unmixed series.

Respecting irregularity in the strength of cast iron, of which the solid pillars experimented upon were composed, Mr. Hodgkinson says, "They were always found to be softer in the centre than in the other parts. To ascertain the difference of strength in the sections of the pillars used, small cylinders, ¾ inch diameter, and 1½ inches high, were cut from the centre, and from the part between the centre and the circumference, and there was always found to be a difference in the crushing strength of the metal from the two parts, amounting perhaps to about one-sixth. The thin rings of hollow cylinders resisted in a much higher degree than the iron from solid cylinders. As an example, the central part of a solid cylinder of Low Moor iron, No. 2, was crushed with 29·65 tons per square inch, and the part nearer to the circumference required 34·59 tons per square inch. Cylinders out of a thin shell ½ inch thick of the same iron required 39·06 tons per square inch, and other cylinders from still thinner shells of the same metal, required 50 tons per square inch, or upwards, to crush them."

(To be Continued.)

On Electro-Chemical Coloring and the Deposition of Peroxide of Iron on Plates of Steel and Iron. By M. Becquerel.

From the London Chemical News, No. 83.

Priestly was the first to obtain colored rings by means of electricity,* by receiving strong charges from a battery, with surface of about two square metres, on metal plates, by means of metallic points directed perpendicularly to their surface.

* *Philosophical Transactions*, vol. lviii.

Nobili, in 1827,* afterwards produced colored rings on platinum, gold, silver, and brass plates, by putting them in communication with one of the two poles of a voltaic pile, plunging them into metallic and non-metallic solutions, and then by directing perpendicularly to their surface a platinum point connected with the other pole. With positive silver, for instance, and solution of sea-salt, he obtained a series of concentric circles surrounded with varied irises, the tints being slightly dimmed by contact with the air. On heating the plates all the rings took a red tint.

I began to study the electro-chemical coloration of metals in 1843,† my chief object being, not to produce colored rings, but to deposit on plates of gold, platinum, copper and silver, thin and uniform layers of peroxide of lead, presenting successively, according to the duration of the operation, which was generally very short, the rich colors of the spectrum. The operation consists in plunging into an alkaline solution of protoxide of lead the piece to be colored, put in connexion with the positive pole of a voltaic pile charged with nitric acid and composed of many layers of plates, and closing the circle with a platinum wire in communication with the negative pole, and of which but the point, which alone touches the alkaline solution, is continually in motion. In contact with the object to be colored, the protoxide of lead, which forms the positive electrode, super-oxidizes, becomes insoluble in the alkali, and deposits itself on the surface in slight adherent layers, producing the color of the thin plates. Air and light gradually fade these colors—a disadvantage I have already mentioned, and which may in great measure be avoided by covering the colored surface with alcohol varnish, which acts but very slightly on the peroxide. With a little practice all the tints desired may be given to a large object with hollows and projections, and each part painted with the appropriate color. These colors are now made fast by the processes to be described further on.

If the solution of protoxide of lead in potash is replaced by a solution of protoxide of iron in ammonia, and the gold, copper, or platinum plate by one of polished iron, there is deposited on the latter a layer of peroxide of iron with red or brown tints, which deepen more and more according to the thickness of this stratum, which never exceeds a certain limit on account of the imperfect conductibility of the peroxide.

In a paper on the precipitation of metals from their solutions by other more oxidizable metals,‡ I stated that on plunging a copper plate into a solution of double chloride of potassium and platinum, heated to 60°, the platinum is deposited and adheres to the copper, producing a platinization, which is quickly acted on by the air, taking first a slightly brownish tint, which becomes darker and darker.

This alteration is partly due to the presence of protochloride of copper, which is deposited simultaneously with the platinum towards the end of the operation. The protochloride may be removed by washing

* *Annales de Physique et de Chimie*, vol. xxxiv., second series.
† *Comptes-Rendus*, vol. xviii. ‡ Ibid.

the platinized copper with water and acetic acid, or by rubbing its surface with cotton and rouge. The alteration is then avoided, or at least does not appear until long afterwards, probably on account of the air traversing the interstices of the platinum, which with copper forms a voltaic pair. The brownish color of the platinum surface is like that which protochloride of copper ordinarily takes under exposure to air and light.

If we make use of platinized copper directly it is obtained from a solution of double chloride as a positive electrode to decompose water with a pile composed of several elements, it produces, under the influence of the oxygen disengaged from the positive pole, peculiar coloring effects, seeing that the tints pass immediately to blue and deep crimson, and the protochloride of copper is not modified by light. While in contact with platinum the latter metal doubtless contributes to the coloration. No similar result is obtained by operating with platinized plates previously treated with acidulated water or rouge. Furthermore, air has no effect on the colors produced—not an unimportant fact, as it will make it possible to obtain fast colors also with peroxide of lead.

Owing to the strata of oxide, the result is the same if heat is gradually applied to non-preserved platinized pieces, but the colors are less brilliant.

If a copper plate with a layer of peroxide of lead giving one of the beautiful colors of the spectrum is employed as a positive electrode to decompose the water, in a few moments it will be found that the coloration is preserved—a result similar to that obtained with platinized copper. By continuing the electro-chemical action for a quarter or half-an-hour, according to the force of the pile, the blue-violet tints fade, turning to green and yellow, as peroxide of lead. The basis of the coloration undergoes no change at the positive pole. It is thought that the acid secondary products formed at the positive pole react on the peroxide and decompose it.

The colored plates thus preserved seem to be in the same condition as iron when it has been plunged in nitric acid, or when it has been used as a positive electrode to decompose this same acid. It is then in an abnormal state, being unattackable by nitric acid.

When a very slight layer of platinum is electro-chemically deposited on a gold or platinum plate by means of a solution of double chloride of potassium and platinum containing no copper, this layer undergoes no change either by the action of the air, or when the plate is employed as a positive electrode to decompose water; but it is otherwise when the solution contains copper. The coloring effects before described are then produced, when the proportion of copper is small and the coloration of the platinum is not destroyed by weak nitric acid—an important advantage practically. In this memoir we have indicated a great improvement in the process of coloration, for by its means more uniform and adherent layers of metals are deposited.

Magnificent coloring effects are produced by a solution of double chloride of potassium and platinum in hyposulphite of soda.

Lastly, deposits of peroxide of iron on iron and steel, which are already almost unalterable by air, will become entirely so when the pieces have been employed as positive electrodes to decompose water.

Comptes-Rendus.

Application of Cyanide of Potassium in Soldering Metals.

From the London Artizan, July, 1861.

In the operation of soldering metals, says Dr. Augustus Vogel, it is very essential to keep the metallic surfaces to be united clean and bright, so that the solder may adhere firmly when in a melted condition. For the purpose of protecting the metallic surfaces from the oxidizing action of the atmosphere, certain fusible substances are usually rubbed on with the solder, and immediately form a thin layer over the surface of the metal. These substances produce, however, not only a protective, but also a reducing action. In practice it is sought to insure these two essential conditions in the choice of the substances generally employed, viz: For soft soldering, resin turpentine, olive oil, powdered sal-ammoniac, mixed either with oil, or with tallow and resin, or a very concentrated solution of chloride of zinc. For hard soldering, borax, or a melted mixture prepared from borax, potash, and common salt, and, in the special case of iron, pounded green glass is generally used. It is well known that the substances above mentioned fulfil to a greater or less extent these two conditions of soldering, viz: deoxidation and protection of the metal from the atmosphere. A material possessing these two qualifications, in the highest degree, would of course best effect this purpose. As the result of a great number of experiments, I have come to the conclusion that the ordinary commercial quality of cyanide of potassium possesses decided advantages in this respect over all other substances. It melts very readily, covering the surface of the metal with a very efficient protective layer, and at the same time is known to exert a strong reducing action, a property which has gained for it many important applications both in technical and analytical chemistry. Cyanide of potassium will be found particularly useful when the surfaces to be soldered cannot be thoroughly brightened. It is difficult, and sometimes impossible, to solder metals when their surfaces are at all corroded, or when they are incapable of bearing the high temperature necessary in this operation, with the ordinary agents, on account of their inferior reducing power; but cyanide of potassium, from its extraordinary energetic action, is able to deoxidize all rusty particles standing in the way of the perfect union of the solder with the metal. The mode of applying the cyanide of potassium in soldering is the same as with borax. Some powdered cyanide of potassium is kept ready at hand in a well-closed glass bottle, and sprinkled over the metallic surface after it has been slightly moistened. In some cases of soldering at very high temperatures, which by practice are soon ascertained, it will be found expedient to use a compound of borax and cyanide of potassium, for the purpose, on the one hand, of increasing by

this addition the small reducing power of the borax, and on the other hand, of diminishing the volatilizing tendency of the cyanide of potassium. Another reason for preferring the employment of this agent is, that during the operation no corrosive vapors capable of acting on the soldering tools are generated, as is the case with chloride of zinc.

Mulley's Auxiliary Steering Apparatus. By W. R. MULLEY, Esq., Surveyor to Lloyd's, Plymouth.

From the Lond. Mechanics' Mag., January, 1861.

GENTLEMEN—Will you kindly allow me space for a few remarks on the present insufficient means of steering steamships of large tonnage, and a brief description of my auxiliary rudder as a remedy.

Rudders and the apparatus for moving them have, since the introduction of steam, occupied considerable attention, and many improvements been made in both. But owing to the immense size of the ships now built, and their extreme length in proportion to beam, together with the effect produced upon the rudder by the screw, the difficulty is greatly augmented; indeed, to such an extent does this exist in some ships, that on a recent trial of one of the finest frigates in our navy, she was, when at full speed, quite beyond all control of her helm.

To remedy such deficiencies, and also to provide a reliable resource in the event of damage to the main rudder, I have devised an auxiliary. It is about half the superficial area of the main rudder, oblong-shaped, and formed of separate bars of copper or iron—according as the ship may be of wood or iron—in such manner as will allow it to twist (its action resembling that of the fin of a fish) in and out of the quarter where it is hung, at an optional depth below the water-line, and when not in use recessed so as to take the shape of the bottom, leaving no projection whatever, and consequently free from the various risks of injury to which the main rudder is liable—that of being shot away in particular, a casualty, now that rifled cannon will be used and directed with so much precision, infinitely more likely to occur than heretofore. The auxiliary rudder would in such cases be quite efficient for the safe navigation of the ship; whereas without some such substitute immediately available, she would be in a far more helpless condition than the loss of her screw would render her, concerning which much has been said and many suggestions made to guard against, as of course while her masts were standing she would still be under control. It would also be highly serviceable for ships, such as iron-cased batteries and others navigating shallow waters and intricate rivers. The auxiliary rudders are worked each by a common steering wheel, placed on either side and near that of the main rudder, and are operated upon simultaneously with it under the same orders, having, in a case of sudden change of helm, the advantage of being brought to act at once, while the main one has first to pass its neutral point. The other part of the apparatus consists merely of a vertical and horizontal shaft, with a pair of wheels and pinion to move a segment-shaft passing

through the ship's side and connected to the after end of the rudder, so that it requires but a small quantity of room from the hold.

Although the auxiliary rudder is undoubtedly calculated to be of greater service to ships of war than merchantmen, yet, as they can be carried without inconvenience, the comparatively trifling cost ought to be no bar to their adoption in long-voyage passenger ships, contributing as it would so much to the security of life. It may be urged that such accidents are of rare occurrence, to which I would reply by asking what amount of money would be thought too much when they do happen for such a help at hand?

Lloyd's Register Office, Plymouth, December, 1860.

On the Preparation of Artificial Coloring Matters with the Products Extracted from Coal Tar. By M. E. Kopp.

From the Lond. Chemical News, Nos. 44 and 46.

(Continued from page 256.)

Preparation of Nitro-benzole.—The preparation of nitro-benzole is accomplished, on the large scale, by allowing a fine stream of benzole and another of the strongest nitric acid to run together in a worm or long glass tube kept well cooled. The two liquids react on each other on coming in contact, heat is disengaged, and nitro-benzole is formed. Commercial nitric acid, mixed with half its volume of sulphuric acid, may be substituted for the concentrated nitric acid. The nitro-benzole collected at the end of the worm, is first washed with water, then with a solution of carbonate of soda, and afterwards once again with water.

Nitro-benzole is a yellowish liquid which at 15° C. has the sp. gr. 1·209.* It boils at 213°, and cooled to 3° it crystallizes in needles. Having an odor closely resembling that of the essential oil of bitter almonds, it has been largely used in perfumery for scenting fancy soaps, for which purpose it has one advantage over the oil of bitter almonds—it is less affected by the action of alkalies. It is almost insoluble in water, but is very soluble in alcohol, ether, and the essential oils. Concentrated nitric and sulphuric acid dissolve it, but it is precipitated on the addition of water. It is decomposed by continued boiling with concentrated sulphuric acid; and under the same circumstances with concentrated nitric acid it forms bi-nitro-benzole. Neither the alkalies in strong aqueous solution nor quicklime act on nitro-benzole; but an alcoholic solution of the alkalies acts energetically, and forms azoxybenzole ($C_{24}H_{10}N_2O_2$). By the action of nitric acid on this last substance a number of other interesting bodies are produced, which, however, it is not necessary now to describe.

Transformation of Nitro-benzole into Aniline.—This is effected by a variety of processes, which we shall proceed to describe in detail.

1. *By means of Sulphide of Ammonium.*—An alcoholic solution of nitro-benzole, after having been saturated with ammoniacal gas, is treated with a current of sulphuretted hydrogen. The liquor now becomes of a deep dirty green color, and deposits a little sulphur. It is

* Stated erroneously by M. Parisel (*Chem. News*, vol. ii. p. 96) to be 1·080.

now left for twenty-four hours, during which time crystals of sulphur are deposited, the odor of sulphuretted hydrogen disappears, and is replaced by a strongly ammoniacal smell. If distilled now to recover the alcohol, a good deal of sulphur is deposited, and it is impossible to continue the distillation long, because of the violent bumping which ensues. It is therefore allowed to cool, and the sulphur is removed. On distilling the liquor again, more sulphur is deposited, which must also be removed. The process must be continued, re-saturating the liquor with sulphuretted hydrogen if need be, until a heavy oily matter (aniline) deposits, which must be separated from the liquor and re-distilled by itself. The aniline is thus obtained nearly pure.

Instead of using an alcoholic solution of nitro-benzole, and treating it successively with ammonia and sulphuretted hydrogen, the alcoholic solution of sulphide of ammonium may be prepared beforehand, and the nitro-benzole poured into it. A part is dissolved immediately, and the remainder by dryness in the course of the operation. It is sometimes advantageous, instead of waiting until the aniline separates, to add hydrochloric acid to the liquor in the retort until it is slightly acid, and then to distil almost to dryness, by which means chloride of aniline is obtained. This is decomposed by an excess of caustic soda, and the aniline set at liberty is distilled off.

To avoid any danger from the bumping, a tinned copper still must be used, which should be heated by steam under a light pressure. At first the temperature should not exceed 90° C., but after some time it may be raised to 100° or 110°.

The ammoniacal alcohol condensed in the worm may be re-saturated with sulphuretted hydrogen, and used over again with a new quantity of the nitro-benzole.

2. *Reduction of the Nitro-benzole by nascent Hydrogen.*—In preparing aniline by this process, the nitro-benzole and zinc are placed in a vessel, and dilute sulphuric or hydrochloric acid is added, so as to produce the disengagement of a small quantity of hydrogen. By degrees the nitro-benzole disappears and aniline is formed, which remains in solution in the hydrochloric or sulphuric acid. To isolate it an excess of caustic soda is added and the mixture is distilled, on which the aniline passes over with the vapor of water.

Bechamp first recommended the employment of acetic acid and iron filings. He places in a retort one part of nitro-benzole, one and a half parts of iron filings, and one part of concentrated acetic acid. The reaction takes place without the application of external heat, the mixture becoming hot by itself, and the vapor being condensed in a receiver, which must be kept well cooled. The condensed products consist of aniline, acetate of aniline, and some unchanged nitro-benzole. These are allowed to cool, and are then returned to the retort and again distilled to dryness. The distillate is now treated with potassa fusa, and the aniline separates as an oily layer, which must be removed and distilled once more.

The residue of the mixture of iron filings, acetic acid, and nitro-benzole, which remains in the retort after the distillation, still contains

a considerable amount of aniline. To obtain this, the retort must be washed out with water acidulated with sulphuric or hydrochloric acid, and the solution filtered, and then evaporated to dryness. The dry residue is then mixed with quicklime, and placed in a retort of iron or refractory ware, and distilled; and the aniline thus obtained must be rectified.

3. *Reduction of Nitro-benzole by Ferrous Acetate.*—Ferrous acetate reacts on nitro-benzole and converts it into aniline, while the sulphate, chloride and oxalate of iron have no action on it. The reaction is represented by the formula—

$$C_{12}H_5NO_4 + 12\,FeO + 2\,HO + \bar{A} = 6\,Fe_2O_3 + C_{12}H_7N + \bar{A}.$$

One part of nitro-benzole is placed in a retort with an aqueous solution of acetate of iron. The retort is then heated over a water-bath for several hours, and then the contents are filtered—being diluted with water if they have become pasty. The residue left on the filter, which is principally hydrated peroxide of iron, is washed with boiling water. The filtrate and washings are then distilled, the condensed products being water, acetic acid, and acetate of aniline. These may be again distilled with strong sulphuric acid (using four-tenths the weight of the nitro-benzole employed), to recover the acetic acid and form sulphate of aniline, and the latter may be decomposed by caustic potash and the aniline distilled off. This process has not been found advantageous, and has consequently been given up.

4. *Reduction of Nitro-benzole by means of Arsenite of Potash or Soda.*—This process was invented by Wöhler. He digested nitro-benzole with a solution of arsenious acid in a strong ley of caustic soda, or placed the arsenical solution in a tubulated retort, heated it to the boiling point, and then allowed the nitro-benzole to fall in drop by drop. Under these circumstances nitro-benzole is transformed into aniline, which distils over, and it is only necessary to saturate with an alcoholic solution of oxalic acid to obtain perfectly pure oxalate of aniline.

The last method of forming aniline we shall quote is that of Schlagdenhausen, who has shown that it is produced when nitro-benzole and sulphide of carbon are heated together in a sealed tube to 160°.

*Properties of Aniline.**—Aniline when pure is a colorless liquid, very astringent, having an aromatic odor, and an acrid burning taste. It is slightly soluble in water, and very soluble in alcohol and ether. Its sp. gr. = 1·028; at — 20° it does not freeze. It boils at 182°, and distils unchanged. When warmed it dissolves sulphur and phosphorus. It is a powerful base, combining with acids to form salts, which in general are soluble. It decomposes ferrous and ferric salts, and the salts of zinc and alumina precipitating from them the metallic oxides. It also precipitates the chlorides of mercury, platinum, gold, and palladium; but it does not precipitate the nitrates of mercury and silver.

Aniline easily oxidizes, turning yellow in water, and in time becoming resinified.

* Gerhardt, *Chimie Organ.* iii. p. 84.

When aniline dissolved in hydrochloric acid is acted on by chlorine, the solution takes a violet color, and on continuing the current of chlorine the liquid becomes turbid, and deposits a brown-colored resinoid mass. On submitting the whole to distillation, vapors of trichloraniline and trichlorophenic acid pass over.

A solution of the alkaline hypochlorites colors aniline a violet blue, which passes rapidly to a pale red, especially in contact with acids.

A mixture of hydrochloric acid and chlorate of potash act on aniline, the final result of the action being choranile $C_{12}Cl_4O_4$, but in the course of the reaction several colored intermediary bodies are formed.*

If a solution of chlorate of potash in hydrochloric acid be added to a solution of a salt of aniline mixed with an equal volume of alcohol, and care is taken to avoid an excess of the hydrochloric solution, a flocculent precipitate is deposited after a time of a beautiful indigo blue; this precipitate filtered and washed with alcohol contracts strongly, and the blue color passes to a deep green. The filtered liquid has a brownish-red color; on boiling it, adding fresh quantities of hydrochloric acid and chlorate of potash, a yellow liquor is obtained, which deposits crystallized scales of chloranile.

An aqueous solution of chromic acid gives with solutions of aniline a green, blue, or black precipitate, according to the concentration of the liquors.

When a small quantity of an aniline salt is mixed in a porcelain dish with a few drops of strong sulphuric acid, and a drop of a solution of bichromate of potash is allowed to fall on the mixture, a beautiful blue color appears after some minutes, which, however, soon disappears.

Dilute nitric acid combines with aniline without altering it immediately; but after some time nitrate of aniline crystallizes in the form of concentric needles, the mother-liquor turns red-colored, and the sides of the capsule become covered with a beautiful blue efflorescence. When a few drops of strong fuming nitric acid are poured upon aniline, it is immediataly colored a deep blue; on applying heat the blue tint quickly passes to yellow, and a lively reaction is manifested, which results in the formation of picric or trinitrophenisic acid.

Potassium dissolves in aniline, disengaging hydrogen, whilst the whole becomes a violet-colored pap.

The other reactions of aniline, which are characterized by the formation of anileine, fuchsine, azaleine, will be related in a future number, when describing the processes for the preparation of these substances. But before commencing these we may just glance at some of the properties and derivatives of binitro-benzole.

Binitro-benzole, as before stated, is formed when nitro-benzole is added drop by drop to a mixture of equal parts of fuming nitric acid and sulphuric acid as long as the liquids will mix. If such a mixture be boiled for a few minutes, it becomes on cooling a thick magna of binitro-benzole, which is easily purified by repeated washings with

* See *Chem. News*, vol. ii. p. 195.

water. A single crystallization from alcohol will furnish the body in long brilliant prisms, which melt at a temperature above 100°, and crystallize again on cooling in a radiated mass.

Binitro-benzole is very soluble in warm alcohol. When a plate of zinc well cleaned is placed in a cold alcoholic solution of binitro-benzole and a hydrochloric acid is added by degrees, we observe that the disengagement of hydrogen, which at first takes place, soon ceases, and at the same time the liquid gradually takes a crimson-red tint.* The reaction being completed, the excess of zinc is removed, and the liquor is saturated by an alkali, which precipitates the oxide of zinc colored a deep purple. The precipitate is collected on a filter and washed with alcohol. By distilling the highly colored alcoholic washings, washing the residue with cold water, then redissolving it in alcohol and evaporating it afresh to dryness, the new matter is obtained perfectly pure. The authors have given it the name of nitrosophenyline. It has the formula $C_{12}H_8N_2O_2$. When obtained as above, it is a black shining substance; when heated, it fuses and decomposes directly; it is almost insoluble in water, but freely soluble in alcohol and acids. An alcoholic solution containing only 0·2 per cent. is so deeply colored that by reflected light the solution seems opaque and of an orange-red.

Concentrated hydrochloric acid and dilute sulphuric and nitric acids form magnificent crimson-red solutions with nitrosophenyline, which is precipitated from them again unchanged by alkalies.

Binitro-benzole treated with an alcoholic solution of sulphide of ammonium is at first converted into nitraniline,—

$$C_{12}H_6(NO)_4N = C_{12}H_6N_2O_4$$

that is to say aniline, in which one equivalent of hydrogen is replaced by one of nitrous vapor. Nitraniline crystallizes in yellow needles, which stain the epidermis like picric acid.

(To be Continued.)

On the Jet.

From the Lond. Mechanics' Magazine, July, 1861.

In a large steamer, lately built at Liverpool, a series of jets were introduced into the chimney in aid of the draft, on the supposed principle which produces such efficiency in the locomotive. In the centre of the chimney, a short distance above the boiler, a two and a half inch tube, forming a circle of two feet diameter, was introduced. In this were drilled sixty orifices, each of a quarter inch area. Through these the steam issued continuously, in a vertical direction, the jets being but one inch apart. That such an arrangement was made without a due consideration of the nature of the jet, or the principle on which its efficiency depends, will be manifest from the following:

Under the conviction that the several jets were so near each other that to a considerable extent they must have neutralized the action

* Church and Perkin, *Quart. Jour. Chem. Soc.* ix. p. 1.

and effect of each respectively, an apparatus of the same dimensions was made and inserted in a tube representing the chimney. The air-metre, hereafter described, was attached so that the entire quantity of air brought in had to pass through it. As no other access of the air was possible, the movements of the metre-vane gave a true measure of the quantity obtained in aid of the natural draft. The following experiments, which were then made, will sufficiently explain the importance of considering the relative position of the several jets, and the value of this metre test.

In the first experiment, the sixty quarter-inch orifices, one inch apart, were all open, the pressure of steam in the boiler being 7 lbs., and which pressure was continued throughout, as shown in the table annexed. The steam having been let in, and a due time allowed for the whole to be thoroughly heated, the number of revolutions of the metre were accurately noted. In this case the revolutions were 540 per minute, and this may be taken as evidence of 540 cubic feet of air brought in aid of the draft.

In the second experiment, each alternate orifice was plugged up, thus reducing the number of jets in action from sixty to thirty, and increasing their distances apart from one to two inches—necessarily with a saving of one-half the expenditure of steam. The result was, an increase in the number of revolutions of the metre (or cubic feet of air per minute) from 540 to 625, as noted in the following table, being a gain of seventeen per cent. in the effective chimney draft, with but one-half the expenditure of steam.

Experiments with Air Metre and Steam Jets.

Revolutions of Metre per minute.		Pressure in Boiler.	Number of Jets.	Size of Jets.	Gross area of Jets.
					Inches.
1-4th-inch Jets,	540	7 lbs.	60	1-4th-inch.	2·945
	625	"	30	"	1.472
	745	"	20	"	·981
	725	"	15	"	·736
	700	"	12	"	·589
1-8th-inch Jets,	740	"	60	1-8th-inch.	·736
	615	"	30	"	·368
1-10th-inch Jets,	700	"	60	1-10th-inch.	·600
	600	"	30	"	·300

In the third experiment, a further reduction in the number of orifices and jets was made, their distances being then three inches apart. The result was a further increase of revolutions and cubic feet of air passing, from 625 to 745, or a further gain of nineteen per cent., with a commensurate saving of steam.

Thus, the increased effect produced solely by placing the jets further apart, say from one to three inches, and thus allowing the several induced currents of air a more lengthened scope for action, was, an increase of draft from 540 to 745 cubic feet per minute, or a gain of

thirty-five per cent., with a saving of two-thirds the quantity of steam. It is needless to dwell on the value of this source of economy.

The next series of experiments with the air-metre consisted in the reduction of the areas or size of the several orifices and jets of steam from one-fourth of an inch to one-eighth, and then to one-tenth of an inch. In each case, as shown in the table, the results were remarkable. We there see that thirty jets of even one-tenth of an inch sectional area, if placed at three inches apart, were more effective than sixty jets of a quarter of an inch when placed within one inch of each other, as was the case in the apparatus as originally constructed.

As regards the economy of steam, the inspection of the last column in the table, giving the gross area for the exit of the steam in each experiment, is sufficiently expressive. This needs no comment. Here we have practical results arising from a just appreciation of the principle on which the efficiency of the jet depends, in opposition to the mere random application of a most useful power in aid of the natural draft in chimneys.

By the use of the meter we come so near to the absolute quantity of air introduced that it practically satisfies all that may be required. The metre consists of an ordinary circular vane, through which the air has to *pass* during the operation, producing motion like that of a windmill, increasing in rapidity in proportion to the strength of the current passing through it. This circular motion of the vane is transferred to a series of dials, similar to those of the ordinary domestic gas-metre. The number of revolutions of the vane are then truly recorded in units, tens, hundreds, &c., up to 100,000, thus enabling an experiment to be continued during above 100 minutes. The vane is eighteen inches in diameter, and having ascertained by a measured cylinder, after a true adjustment of the leaves, that the quantity passed during a given number of revolutions was equal to one cubic foot for each revolution, it was taken as a sufficiently reliable datum for calculating quantities. The number given by the index dials, then, will represent the number of cubic feet that have *entered* a furnace or been *drawn out*, as the case may be, or have escaped by the chimney, under the different circumstances of high or low temperature, with partial or complete combustion.

The vane is hung with such delicacy, and is so easily set in motion, that the loss by friction of the machine need scarcely be taken into account.

By the use of this apparatus, numerous practical errors have been corrected touching the quantities of air required, first, for the combustion of the *coke portion* of the coal resting on the bars in the furnace, and passing through the ash-pit; and, secondly, the quantity required for the combustion of the *gas portion* in the chamber, the appearance of smoke giving unquestionable proof that the requisite amount of air had not been introduced.

Of the value of the jet system, and the important service it may be enabled to perform, it is important to consider the insufficiency of the natural draft in reference to the quantity of air required in large

boiler furnaces, and in effecting perfect combustion. For this purpose it must be borne in mind, that the quantity of air absolutely required for the combustion of the gas portion alone of bituminous coal—as, for instance, the Newcastle—is at least one-half that required for the coke, or solid portion of the same. In a word, if the coke of a ton of coal requires 200,000 cubic feet of atmospheric air, the gas of the same ton weight will require 100,000 cubic feet—a fact but little thought of by boiler-makers; yet without this full equivalent of air much of the gas portion is not only wasted, but, by its conversion into smoke and soot, seriously impedes the action of the boiler, and in particular the tube portion of it. This latter, or 100,000 cubic feet, must be supplied to, and mixed with, the gas generated *above the fuel*, in the chamber of the furnace; while the 200,000 cubic feet have to be supplied by the *ash-pit*, and pass up through the solid fuel on the bars.

Now, where the heat-absorbing surface is well applied in the generation of steam (which is rarely the case) in marine boilers, the natural draft is invariably deficient. Here then we have, by the aid of an artificial draft, through the medium of the jet system, the direct means of effecting the perfect combustion of the whole of the combustible portion of the coal. That this is imperfectly understood, or but little thought of, by those who construct such boilers, we have an absolute proof in the generation and evolution of that mischievous nuisance, the dense volume of smoke, which characterizes all steam marine boilers using bituminous coal.

Finally, we may conclude that the efficiency of the jet system depends mainly, if not exclusively, on the amount of induced current, combined with a judicious arrangement of the several jets, keeping them in harmony with the area of exit of each, the pressure to which it is subject, and the space of freedom allowed for the several induced currents to exercise their maximum effect.

On the Composition of Cast Iron and Steel. By M. E. Fremy.

From the London Chemical News, No. 78.

(Continued from page 262)

These bodies were reduced to very fine filings, and this metallic powder, freed from all foreign matter, was submitted at a red heat to the action of dry hydrogen. In these three trials, the filings during the whole of the experiment disengaged considerable quantities of ammonia. This experiment leaves no doubt on the subject, and proves that nitrogen, contrary to the hitherto received opinions, is a constituent of steel. Steel is not, then, a simple carburet, but a nitro-carburet of iron.

If I be not deceived as to the bearing of my inquiries, it appears to me that they ought to have a certain influence on the metallurgic operations relating to the manufacture of steel. Thus, in the cementation of iron, all the conditions appear to be realized which give to the metal not only carbon but also nitrogen. It is probable that the

different qualities of steel depend on the duration of the cementation, and also on the relative proportions of the two elements which combine with iron. In the preparation of steel by puddling, it is equally important to determine what are the varieties of iron most likely to yield the proportion of nitrogen required for the constitution of steel; or those which, not containing sufficient, require to have it imparted while being converted into steel.

I have spoken of a steel which has for bases carbon and nitrogen; but this compound is not the only iron alloy the composition and properties of which are of interest to metallurgical industry. It is probable that bodies bearing some analogy to carbon or nitrogen would serve also in the production of steels. Is it not already known that granulated iron, which is harder than ordinary iron, and which in some respects is allied to steel, is principally obtained by the reduction of minerals containing phosphorus? If the combination of iron with carbon and nitrogen ought to be taken as the type of steel, it would be very curious to determine the modifications undergone by iron when carbon or nitrogen are replaced by other simple bodies. I shall devote another communication to this interesting subject, showing that there are many steels, and that they form a whole group of compounds, each of which ought to be successively examined.

It appears to me that the following conclusions may be drawn from the new facts which I have communicated to the Academy:—

1. In studying the successive or simultaneous action of nitrogen and carbon on iron, ammonia may be used advantageously to furnish nitrogen, and lighting-gas carbon; the chemical reactions thus produced by gases, yield pure compounds; they can be easily observed and regulated.

2. When iron has been submitted, not for too long a time, to the action of ammoniacal gas, it produces no crust of nitride of iron, it is simply nitrogenized, becomes then of a zinc white, partly preserving its malleability, and resembling a real alloy.

3. Iron heated in a current of lighting gas, immediately becomes carburetted and transformed into a very soft, grey, graphite-like, fusible cast iron, which is suitable for the finest castings. Steel is never formed in this reaction of lighting gas on cast iron.

4. Steel is formed by the reaction of carbon and nitrogen on iron.

5. Pure iron which, under the influence of lighting gas, is transformed into very fusible cast iron, is previously nitrogenized, loses its fusibility, and is converted into steel by the action of the gas. Fragments of the same metal have been nitrogenized for different lengths of time, and then submitted to the action of lighting-gas; those retaining a small proportion of nitrogen were very incompletely converted into steel; those, on the contrary, which were strongly nitrogenized, formed a beautiful steel. It is, then, in some measure, the quantity of nitrogen contained by iron during carburation which determines the degree of acieration.

6. It does not seem to me possible to admit that cementation may be produced exclusively by a carburetted volatile body, since lighting

gas acting at red heat on iron forms only cast iron, while by the previous presence of nitrogen in the metal, it is immediately converted into steel.

7. When iron is transformed into steel, the nitrogen is not eliminated by carbon, for I have ascertained that all the steels of commerce are nitrogenized, and disengage an abundance of ammonia when submitted to the action of dry hydrogen.

8. All these facts tend to the following conclusion, which is a condensation of my paper:—That steel is not, as it has hitherto been considered, a carburet of iron, but rather a nitro-carburetted iron. I have adopted the title of nitro-carburetted iron to explain the composition of steel, because it well expresses my opinion of the composition of this body, in which such small proportions of metalloid so completely modify the properties of iron.

The paper having been read,

M. Dumas rose and congratulated M. Fremy and the Academy on the important results which must flow from the labors of the author of the paper. The theory of the production of steel seemed henceforth determined, and it might reasonably be hoped that great practical results would ensue. Who, for instance, did not forsee—and it was for M. Fremy to follow out the demonstration—that great advantage would result from these new, methodical, regular, and certain processes, when there was occasion either to case-harden the surface or edge of certain iron implements or instruments? After having forged, filed, and finished them off in the state of iron, a current of ammoniacal and carburetted gases would convert them more or less completely into steel. The depth of the stratum of steel being regulated by the duration of this gaseous cementation with a certainty never obtainable by cementation with powders, or by the use of horn or animal matters in the empirical processes. The Academy could but congratulate M. Fremy on his present success, and on his disinterestedness in publishing his important labors.

M. Morin remarked that M. Fremy's researches explained numerous empirical receipts and processes for the cementation of steel. In most of these processes, mixtures were employed containing various proportions of carbon, and of more or less nitrogenized substances, such as ammoniacal salts, horn shavings, leather cuttings, soot, &c., &c., the result being a cementation more or less deep, according to the use to which the instruments are to be applied. He thought it necessary also to observe that the character of steels produced by different methods varied greatly, not only where these methods differed, but also with almost identical processes. Moreover, certain kinds of steel, and it seems particularly those obtained by puddling, after undergoing many energetic fagotings, appear to be susceptible of losing their characteristic properties of hardness and elasticity acquired by tempering, and to acquire a considerable resemblance to the most ductile irons. Lastly, the cast steels produced by the new processes of fabrication, when properly forged, possessed an elastic resistance

capable of undergoing a much greater strain than those manufactured by the ordinary methods.

Besides the foregoing speakers, several members of the Academy joined in the discussion when M. Fremy's paper had been read, and, among other remarks, it was stated that the composition of cast iron might be different to that of steel. M. Chevreul then made two observations, one respecting black cast iron, and the other on the composition of steels.

1. *On Black Cast Iron.*—At the end of the last century (1799), Proust observed that, when treated with weak sulphuric acid, black cast iron yielded an oily matter, a portion of which was carried off by the hydrogen gas and made the tubes of the apparatus greasy, while the other portion remained mixed with the black residue, from which alcohol could extract it. I never neglect an opportunity of quoting this observation of my illustrious fellow-citizen, as an example of the possibility of producing, by chemical forces, compounds analogous to those of organic nature. Experience has long since proved that aqueous vapor by reacting on charcoal, yields, besides carbonic acid, or oxide of carbon, nothing but hydrogen, and not carburetted hydrogen, as hitherto believed:* the combination of the cast steel with the nascent hydrogen seeming to me difficult to admit; this has led me to conjecture that in Proust's experiment the water might assist in the production of the oily matter simultaneously with the carbon and hydrogen. Now, M. Fremy's observations seem to throw a light on the subject, by indicating that it is not carbon, as was represented, which yields the oily matter.

2. *Composition of Steels.* — Independently altogether of science, two bodies possessing different properties have never been confounded; so that when an iron was observed which hardened on being suddenly cooled, it was distinguished from one preserving its original ductility after undergoing the same cooling influence. Thenceforward, the name of steel was bestowed upon the first substance to distinguish it from what is properly called iron; or, in other words, between steel, which tempering hardens, and iron, which tempering does not harden. Since the time of the revival of chemistry, the difference between steel and iron was attributed to the presence of about a thousandth part of carbon in the former. Later, the influence of various bodies on steel was recognised. Berthier mentions chromium; Faraday and Stodart aluminium, platinum, and its accompanying metals; but the fact which to me seems of the greatest importance, is the method by which MM. Faraday and Stodart obtained from cast iron some centièmes of iridium and osmium, which when analyzed yielded no trace of carbon. Setting aside the question whether steel is an indefinite compound of iron and one or several simple bodies distributed through the whole mass of the steel, or whether it is a definite compound of iron with one or several simple bodies distributed in indefinite proportions in the iron in excess of the elements of the definite compound, I conclude, from the whole of the facts I have stated, that in a chem-

* "Eighth Lesson on Chemistry Applied to Dyeing," pp. 23, 24.

ical treatise steel in general must be regarded not as a definite compound by the nature of its constituent parts, but as a particular state of iron produced by the union of this metal with bodies the nature of which is variable; and it is from this point of view that, after defining steel, independently of all scientific considerations, as an iron which is hardened by tempering, I discriminate in my fourteenth lesson on "Chemistry applied to Dyeing," published in 1829, p. 78:—

1. Steels formed by iron and carbon;
2. Steels formed by iron, carbon, and a third body;
3. Steels formed by iron and some other body, which is not carbon; or uncarbonized steels.

The results of M. Fremy's interesting experiments are, it seems to me, easily connected with what is already known of steel, if looked upon from the point of view I have explained, instead of regarding them in the general way. It is now important to find out—1, Whether it is true, as Guyton says, that diamond dust will turn iron into steel; 2, Whether acieration can be effected without the intervention of nitrogen.

M. Fremy said, in reply to the preceding observations, he was happy to observe the interest the Academy took in his inquiries on steel, and he thanked its members for the friendly way they had spoken of his labors. It was not his intention to treat, in the present communication, of the influence exercised by nitrogen and carbon on the properties of iron, but all questions relating to the fabrication of steel and cast iron had for a long time been carefully studied in his laboratory, and the results would by-and-by yield materials for papers bearing on the following points:—

1. The relative proportions of nitrogen and carbon required to be introduced into iron to form good steel;
2. Circumstances opposing acieration or altering the qualities of a steel when formed;
3. The mode in which carbon penetrates the metallic mass;
4. The reason why such small quantities of carbon and nitrogen can convert iron into steel;
5. The study of steels containing metals such as manganese, chromium, tungsten, aluminium, &c.;
6. Classification of different kinds of cast iron; examination of the effect which silicium, phosphorus, arsenic, and sulphur exert upon these compounds; study of the cast iron best suited to the fabrication of puddled steel.—*Comptes-Rendus.*

On a Standard of Length By Henry Hennessy.

From the London Athenæum, June, 1860.

Mr. Taylor's letter, in your last number, has called to mind certain points which I had omitted in my communication of May 5. The calculations referred to by Mr. Yates in his "Essay on a Standard of Length," were deduced by me from the results and formulæ of Bessel, in his "Memoir on the Elliptic Spheroid of Revolution which most

nearly corresponds with the Existing Measurements of arcs of the meridian," "Taylor's Scientific Memoirs," vol. ii., and "Astronomische Nachrichten," No. 333. As my results have not been published, I may be permitted to refer to such as bear upon the matter under discussion. I assumed the one-hundred-millionth part of the earth's axis as the proposed standard of length, instead of the forty-millionth of a meridian, which constitutes the French metre. I found the length of the proposed metre to be about 5·004774 English inches; in other words, I made the polar diameter of the earth to be about 500,477,400 instead of 500,491,440, referred to by Mr. Taylor and Sir John Herschel as the result obtained by the Astronomer Royal. My result makes the millionth part of the earth's axis still closer to five hundred inches than that to which attention has been called in your pages. At the time I communicated my calculations to Mr. Yates, I was not so much struck by the coincidence between the proposed metre and the British inch as to call particular attention to the matter. Although the circumstance appeared interesting and valuable, I still regarded it as only a coincidence; and I fully concur with the judicious remarks of Col. James, in the *Athenæum* for May 19, as to the desirableness of not attaching exaggerated importance to accidents of this kind.

A subject immeasurably more important is touched on by Mr. Taylor, when he speaks of the advantage of saving the country from the French metre. In so far as the metre is less perfect, as a scientific result, than the fraction I have deduced from the earth's axis, I concur with him; but in so far as it is merely French, I cannot assent to his conclusion. Many years after the publication of the *Principia*, it was a point of national honor among a certain set of French *savans* to prefer the vortices of Descartes to the so-called British system of gravitation. Have we all an equal right to smile at this kind of patriotism?

But if the metre happens to be French on account of its derivation from a measurement across France, it is not at all true that the system of measures associated with it is essentially French. I refer to this because any remarks touching the metre are frequently applied to the whole metrical system. The decimal arrangement of that system is as much common property to the mental habits of the members of the ten-fingered human race as the earth's axis is common to the various countries on which they exist. I hope, therefore, that questions of nationality will be as completely excluded from discussions of this kind as they have long happily been from other portions of the exact sciences.

Wynnefield, Rathgar, June 5.

Improved Steam Engine Indicator.

M. Bourdon has successfully applied the flat bent tube of his steam gauge to a steam engine indicator, the action of which is, we learn, all that can be desired. The card is carried on a flat plate which is made to rise and fall at each double stroke of the piston.

On the Unit of Length. By James Yates.

From the Lond. Athenæum, June, 1860.

Although the adoption of a certain portion of the earth's axis as the Unit of Length was suggested at the very first meeting of the International Decimal Association at Paris, in September, 1855, and was considered and discussed at a meeting of the British Branch of the Association at London, in 1857, yet the reproduction of this proposal in two specific forms, by so distinguished a philosopher as Sir John Herschel, seems to require some remarks in vindication of those who still adhere to the "boasted Metrical System of our French neighbors."

The two units, proposed by Sir John Herschel, are the 500 millionth part of the earth's axis, equal to 1·001 inches nearly, and the 10 millionth of the same, equal to 50·05 inches nearly. As the author does not appear to aim at any international system, I shall endeavor to meet him on his own ground, and to consider only the objections applicable to his units for English purposes. These are, their inconvenience and their inaccuracy.

First, their *inconvenience.*

One of them, *the inch*, is much too small: the other, which may be called *the ell*, is too large. A unit of length ought, if possible, to be such that it will represent that length, or something not very far from that length, which is more commonly used than any other for linear measurements. Supposing the system of measures to be decimal, the unit ought to be as nearly as possible at the middle point between the highest and lowest denominations, so that the numbers may increase as much by decimal multiplication in ascending as they diminish by decimal division in descending. Now, an inch might require to be divided in some rare cases into 100, or even 1000 parts, but, going in the other direction, it would require to be multiplied by 1000 times 10,000, and thousands of thousands, so as to require rows of figures which would be unmanageable. The lowest unit of length ever used for general purposes, has been the palm of 8 or 10 inches, used in Italy. Mr. Whitworth, of Manchester, has taken the British inch as a unit for a peculiar class of English manufactures requiring only very minute subdivisions, and many of his brother machinists have followed him. But for all the ordinary purposes of the shop and the market, as well as for the calculations of engineers, architects, surveyors, astronomers, and many other trades and professions, such a unit would be intolerable.

On the other hand, Sir John Herschel's ell would be too long. There cannot be a better test of the fitness of such a measure to be generally useful than the trial of it in measuring cloth. The length of this ell, of more than 50 inches, would require a constant effort in stretching out the arms, which would be fatiguing to men, and still more to women. The best unit for ordinary purposes, being adapted for common every-day operations, must be seen at a glance of the eye, easily handled, easily carried from place to place. I believe no better length

than the metre was ever contrived for these purposes. A unit of length so great as that proposed by Sir J. Herschel, has seldom, if ever, been used by any nation, and, I apprehend, the reason is, that it is not adapted to the frame of man. Sir Charles Pasley, though he is no admirer of the metre, admits its superiority in this respect. "It is," says he, in his Memoir read at Cheltenham, "a more convenient measure for cloth than the old French *aune*, which was rather too long for that purpose." Yet this "old French *aune*" was shorter by above 3 inches than Sir John Herschel's proposed measure. The importance of the same view of the question is recognised by the Commission appointed last year by the Imperial Academy of St. Petersburg, who observe, in reference to the idea of making the English foot the basis of a decimal system, "Le pied est trop petit pour mesurer des étoffes; une mesure de dix pieds serait trop grande."

Secondly, The *inaccuracy* of the proposed new units. After all the objections urged against the metre on the ground of inaccuracy, it was little to be expected that accuracy should be disregarded by its opponents, or that they should propose any new unit which does not surpass the metre in this respect. Yet Sir J. Herschel's units are inaccurate in a double sense. He compares them, on the one hand, with the English linear measures, and, on the other, with the supposed length of the earth's axis: they do not exactly correspond with either; yet he treats the difference as a matter of no consequence. The difference of his new inch from the English inch is 1 in 1000; supposing this thousandth part to be added, its difference from the 500-millionth of the earth's axis would be 1 in 106,000.

Although we ought to aim at absolute identity in weights and measures, and to avoid a mere approximation as tending rather to augment confusion than to abate it, I am disposed to agree with Sir John Herschel, that so small a difference as 1 in 106,000, or even 1 in 1000, is of no practical importance, and would not be estimated in the ordinary operations of trade and commerce. I only say of his new standards and of the metre, "Trutinâ ponantur eâdem." I make this concession, even although the practical difference would be in reality two or three times as great as 1 in 1000, because the operations of trade consist in the vast majority of cases in weighing, and in solid or superficial, not linear measurements, and in all these cases we have to do, not with straight lines, but with the squares and cubes of those straight lines. It ought also to be remembered, that the supporters of the metre have never asserted its absolute accuracy as the 10-millionth part of the quadrantal arc; they have never discouraged or opposed any operations or inquiries which might lead to greater precision in determining the figure and dimensions of the earth; and they have provided by the alteration of the standard temperature of the original metre for any change in its length which may be found necessary.

With respect to the question of taking the earth's axis as a basis of calculation instead of a meridian, I am compelled to dissent from those who prefer the more novel course. If the axis is taken, it is calculated either from a medium or average of two or more meridians, or from

one meridian only. General Schubert thinks it best to assume one meridian only. If this be done, there is an end of the argument founded on universality and the absence of national predilections. Some of the English have opposed the metre, because it was computed from an elliptic meridian passing through France;* Schubert prefers the Russian meridian, and having computed from it alone, and then from the Indian meridian alone, the length of the axis, he combines the two computations, but gives to the Russian twice the weight of the Indian. Thus the axis is exposed to objections of the same kind as the meridian. Sir J. Herschel indeed asserts that the axis is "the more primary and fundamental unit." But, although this may be true in reference to the formation of a circle or ellipse in abstract mathematics, or according to some metaphysical conception, yet in the present instance the contrary is the truth. The periphery, or a definite portion of it, must be ascertained first, and from this calculation the axis must be deduced.

All the most recent discussions, not only those of Col. Everest and Sir Henry James, but even those of Schubert, Airy, and Sir John Herschel himself, seem to me to prove that the exact form and dimensions of the earth, and more especially its oblateness, and the length of its axis, are not yet determined with perfect accuracy. The emphatic words which form the very first sentence of the "Base du Système Métrique," are still true, viz: that "the two questions of the earth's size and its figure, which have so long employed astronomers and geometers, appear to be of such a nature that they will never be entirely set at rest." The assumption that the earth is an exact ellipsoid of rotation, and that all its meridians are true ellipses, equal to one another, is contrary to all evidence, both geological and geodetic. As to the superiority of the Russian arc, I presume there will be no dispute. The French arc was taken when the Metrical System was invented, for good, solid reasons, which had not their foundation in any national or exclusive partiality. This arc lay partly to the north and partly to the south of the mean parallel of north latitude, 45°, so as to admit of a good comparison with the indications of the pendulum; it was longer than any which had then been ascertained, viz: $9\frac{1}{2}°$; its northern and southern extremities were in contact with the sea; and, being the most recent measurement, it might naturally be supposed to have had the advantage of all improvements which the course of time had introduced. All these arguments, except the first, are now applicable to the Russian arc, the length of which is 25° 20′, extending from the mouths of the Danube to the Arctic Sea. The friendly communications which have taken place between the British branch of the International Association and many of the most distinguished Academicians of Paris and Brussels on the one part, and the Academy of Sciences at St. Petersburg on the other, afford a decisive and pleasing indication of a willingness on all sides to accept the Russian arc; and the same generous disposition is conspicuous in the great

* Sir J. Herschel speaks of it as "passing through *Paris*," which is a mistake; it lies considerably to the east of Paris.

work which does so much honor to our country, the latest publication on the Ordnance Survey of Great Britain.

But does it follow, because the Russian Academicians have thus far succeeded in making the best measurement of a meridian, that the metre is to be relinquished? By no means. The labors of Struve and his coadjutors in Russia, no less than those of Bessel in Prussia, and of Lambton and Everest in India, tend on the whole to confirm the conclusions of Méchain and Delambre in France; because the lengths of their meridians all come within a wonderful degree of proximity, and, in fact, present no difference which is important in regard to common affairs, or in respect to the intercourse of nations for commercial and political purposes. The Commission, lately appointed by the Imperial Academy of St. Petersburg in consequence of the application made to them by the International Association, and consisting of MM. Kuppfer, Jacobi, and Ostrogradski, have reported in favor of the metre, and many of the best-informed and influential men in that country are ready to support their opinion. This may lead to a general agreement upon the standard temperature at which the metre shall be taken in future. It may be found expedient to declare, by the universal consent of those who wish for an international system, that instead of requiring the metre to have, when used as an exact standard, the temperature of melting ice, it shall be 1, 2, 3, or any other number of degrees higher. Thus strict science may be satisfied together with philanthropy. The change may be made, although not an individual, except here and there a solitary astronomer, knows that there is a change. The 10,000,000th part of the quadrantal arc, whether it be deduced from the French or from the Russian measurement, and represented, as it now is, by a bar of platinum, will scarcely differ by an amount equal to the thickness of a film of varnish upon the extremities of the instrument. The whole question, so far as regards practice, is a mere scientific refinement.

May, 1860.

A New Method of Decomposing Hypochlorite of Lime, in order to effect what is known as the "Discharge."

From the Lond. Practical Mechanic's Journal, September, 1860.

A few years since, M. Steinbach, director of the establishment of Messrs. Steinbach and Koechlin, instituted some experiments in the hope of obtaining some improved process for effecting the bleaching and the discharging of dyed fabrics. A solution of bleaching powder was printed upon pieces of cloth, which were subsequently dried by being passed over a heated cylinder, in order that the hypochlorite of lime should become converted into chlorate of lime and chlorate of calcium. The experiments were most successful, and constituted an important progression in the bleaching and discharging of fabrics. In applying this process to madder-dyed fabrics a great inconvenience arose, in that pieces dyed in reds or pinks were sensibly discolored. To overcome this great defect, hypochlorite of zinc, or rather those products which result from the decomposition of hypochlorite of lime by means of sulphate of zinc, are employed. The *modus operandi* is

the following:—A mixture composed of water, 100 parts, by weight; gum, 50 parts; sulphate of zinc, 40 parts; is printed on the dyed fabric, and when dried the piece is immersed for two minutes in a cold bath, consisting of a solution of hypochlorite of lime, marking two degrees Baumé.

This new process for effecting the "discharge" is much more rapid, more certain, and very much more economical than the old method; the difference being the value of sulphate of zinc compared with that of tartaric acid.

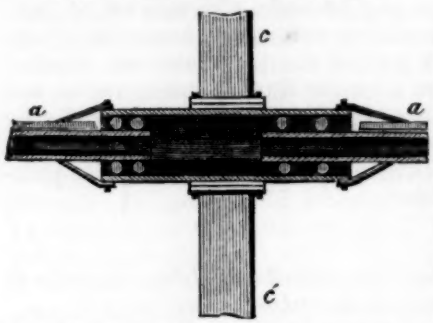
The Tooley Street Fire.

From the London Mechanics' Magazine, July, 1861.

SIR:—Referring to the lateral pressure of the cast iron beams on the side walls in the great fire in Tooley Street, causing their falling and the loss of many valuable lives, I wish, through your valuable and influential journal, to recommend, where iron beams and brestsumers are used, to arrange them so as their expansion and contraction may occur by their ends bearing on rollers in an open socket or tube, having a blank space behind the end of the brestsumer in the socket, to permit its receding inside the tube or socket in case of fire or great heat, and to contract where affected by severe cold.

I am, Sir, yours truly, GEORGE WALCOTT.

24, Abchurch Lane, London, E. C., July 16, 1861.



P. S. A socket or tube containing rollers might be let into the side walls instead of being fitted to the pillar supporting the floor. *a*, *a*, is the floor. *b*, is the cornice closing floor, and permitting the floor to roll into the socket or tube. *c*, a pillar supporting an upper floor; *c'*, a pillar supporting the floor beam. *d*, is a tube or socket open at both ends for receiving and allowing for expansion or contraction.

The Pyrometer. By M. A. NOBEL.

From the Lond. Practical Mechanics' Journal, September, 1860.

Serious difficulties have hitherto presented themselves in the construction of a really exact pyrometer. M. Nobel, an engineer at St. Petersburg, has long been engaged in the study of the heat developed by furnaces of various kinds. As the result of his experiments, he has recently brought out a very simple apparatus, most easy of application and exact in its indications. This apparatus consists essentially of a cylindrical vessel or chamber, composed of platinum or other refractory substance, capable of withstanding a considerable degree of heat. This chamber is connected by a tube with a pressure indicator or manometer. Bourden's is found to answer well. The vessel is

placed in the furnace, and the tube is passed through a luted opening in the side thereof, and is connected at its outer end with the manometer. The inventor of this apparatus states, that bearing in mind that at a temperature of 1600°, the pressure of the air contained in the chamber upon the manometer would be about equal to six atmospheres, he suggests that by reason of the high temperature of the vessel, inconveniences may arise, and that it would be advisable to employ a rarified gas, which would give the following results at the temperatures given below:—

½	atmospheres,	0° cent.
½	"	266° "
1¼	"	1600° "

M. Nobel also remarks, that the gas contained in the external pipe not being at a temperature equal to that in the chamber, errors may arise; but, by proper calculation, these errors may be rectified, and considerably reduced by reducing the diameter of the pipe, to the smallest possible dimensions. The length of the pipe will depend upon the application of the apparatus. If the pipe is very short outside the apparatus of which the temperature is to be ascertained, it may be kept cool by a water-bath. By using a very sensitive Bourdon's manometer, and by adding thereto an apparatus for showing the number of revolutions of the needle or indicator, the increasing temperature for each degree will be readily arrived at, allowance being made, of course, for the atmospheric pressure on the apparatus itself. In practice, however, such allowance has not been found absolutely necessary.

FRANKLIN INSTITUTE.

Proceedings of the Stated Monthly Meeting, October 17, 1861.

John Agnew, Vice President, in the chair.

Isaac B. Garrigues, Recording Secretary.

The minutes of the last meeting were read and approved.

A letter was read from the Royal Society of London.

Donations to the Library were presented by the Royal Geographical Society, the Statistical Society, the Zoological Society, the Society of Arts, and the Commissioners of Patents, London; R. H. Lamborn, Esq., Altoona, Penna.; the Philadelphia Board of Trade, John C. Trautwine, Esq., and Prof. John F. Frazer, Philadelphia.

The Periodicals received in exchange for the Journal of the Institute, were laid on the table.

The Treasurer's statement of the receipts and payments for the month of September was read.

The Board of Managers and Standing Committees reported their minutes.

Candidates for membership in the Institute (9) were proposed.

A. L. Fleury, Esq., of Philadelphia, made the following communication relating to a new process of refining iron and steel by induced electricity.

At the present time, when iron in its various forms enters so largely into the economy of life, holding a high place in the interests of Pennsylvanians, it would be difficult to exaggerate the importance to them of any discoveries simplifying and cheapening the processes of its manufacture. It is with this feeling that I take the liberty of addressing this communication to the Institute, joined also to the hope that iron manufacturers will give the subject due consideration.

Since the year of 1806, when Sir Humphry Davy in his Bakerian lectures, delivered before the Royal Institute in London, at first demonstrated the decomposition and recomposition of water, and other substances which were believed to be elementary, by the simple agency of electricity, we have received ample and most conclusive proofs that in this modification of power, in electricity, we have the most effective decomposing agent, and are by it enabled to destroy the chemical affinity with which impurities cling to other substances. Sir Arthur Wall of Birmingham, England, was the first who used galvanic electricity for the purification of iron. Some twelve years ago, he fully demonstrated the value of electricity as a refining agent, and had it not been for the trouble, expense, and danger, connected with his process, it would doubtless have found a speedy introduction. Mr. Wall used a large number of Smee's batteries and polar platina plates, in order to precipitate the positive impurities on the negative pole while the metal was in its melted state, in a manner similar to the ordinary galvanizing process. I had been experimenting for some time on the influence of electricity on metals, with the view of simplifying the application of this important agent.

The use of the now celebrated Ruhmkorff's induction coil (an apparatus not yet discovered at the time when Mr. Wall secured his English patent) presented some new and different effects from those of the ordinary galvanic battery. While the galvanic current destroys the old and produces new chemical affinity, the interrupted secondary current simply destroys the same, without producing the before-named effect of the continuous current. Before describing my experiments with induced electricity, I have to digress to another subject, which, though it may seem somewhat abstract, is still closely connected with the same.

Wherever we notice a cellular or fibrous texture in organized matter, we invariably find the presence of nitrogen. In the plant as well as in the animal fibre, nitrogen appears as the most necessary constituent. Why should not nitrogen have something to do with the fibrous condition of metals? The celebrated chemist Fremy, in Paris, has lately proved beyond a doubt that nitrogen is a necessary constituent of steel; I may perhaps succeed in proving that nitrogen is also necessary for the formation of the tenacious fibre in iron and other metals. I simply mention here this abstract idea because it seems to be somewhat connected with the experiments which I come now to describe.

Through the kindness of Prof. Frazer, I had procured a suitable induction apparatus, made by Mr. Ritchie in Boston, and assisted by Mr. Adams, a practical English iron manufacturer, I began a series

of experiments in one of the neighboring iron works. One of the best experiments was on about 3000 lbs. of old cast iron, similar in quality to some which has been sold in New York for $12 per ton; of which 900 lbs. at a time were placed in a double puddling furnace, without cinders, and heated to the usual degree. The broken secondary electric current was passed through the heated metal from side to side by means of two platina points, for about ten minutes only, during the stage of fermentation—the so-called "coming to nature" of the iron. At the same time, I introduced some nitrogenized hydrogen in the form of a small quantity of carbonate of ammonia. The heat seemed to increase considerably, and the electricity passed in dense multiplied sparks from side to side. I found by several successive experiments that the impurities of the iron, freed from their former combination, boiled up, and, meeting the nitrogenized hydrogen, were carried off, either as volatile cyanides, or in various hydrogen combinations, leaving a fine fibrous iron in the furnace.

(For the introduction of volatile salts or liquids into the iron, I contrived a hollow working tool, for the use of which, as also for the electric process, I have obtained letters patent.)

The iron after having been balled, was passed through the well-known Bordan's squeezer, rolled, *without reheating*, into plates, and finally cut into nails. This would give to the manufacturer a saving of about $5 per ton.

As I have made arrangement for the introduction of these and other improvements in the manufacture of iron and steel, in one of the largest works in this State, I shall be able to continue my experiments and communicate the results to the Institute.

Mr. W. Jones called the attention of the members to several specimens of rust taken from various parts of a government steamer's boilers, working in connexion with engines having surface condensers, and stated that Chief Engineer Wm. H. Shock, U. S. Navy, would make a few remarks in relation to the same.

Mr. Shock remarked that the specimens of rust referred to were from various parts of a marine boiler, working in connexion with engines having surface condensers, and showed the destructive character of some agent present in the boilers. He did not expect on the present occasion to do more than invite attention to the subject, in hope of exciting a spirit of inquiry, and eliciting investigation upon this very important question. His attention was first called to it some months ago, since which time he had diligently sought for the cause producing so serious effects. A Board, of which he was a member, had been appointed by the government to investigate the subject, but it was not yet ready to report.

He stated that a variety of opinions existed as to the cause of the serious deterioration of the boilers from which the specimens of rust before him had been taken, and a careful examination had led to the conclusion that it was owing to the presence of copper, and briefly explained his reasons, reserving a more lengthy and elaborate explanation of his theory for a paper he proposes to offer at some future day.

Mr. J. W. Nystrom exhibited to the meeting an improved Barometer, which consists of a glass tube, *a b*, of about 7½ feet long, with a bore, slightly tapered, of about 0·07 of an inch mean diameter. The tube is closed at the smallest end, and filled with a column of mercury, *c d*, of about 32 inches in height, in the following manner. Place the glass tube in either an inclined or a vertical position, with the small or closed end downwards; run a fine iron wire down to the bottom of the tube, and pour in the mercury through a paper funnel; the air in the tube will escape along the wire, while the mercury will fall to the bottom. Draw the wire gently out, moving it forwards and backwards, so as to give time and opportunity for the air to escape with the wire; then place the glass tube in a vertical position with the small or closed end upwards, when the column of mercury will fall until its height balances the atmosphere. As the tube is larger at the lower end, the column of mercury becomes shorter the lower it falls; the rise or fall of the mercury is occasioned by different atmospheric pressures, and the range of the column in moving up and down is wholly dependent on the difference in diameter of the tube at the upper and lower ends. The scale is placed at the upper end of the tube, so that the top of the column indicates the height of the barometer. The scale is graduated either graphically by comparing it with a standard barometer, or by actual measurement of the column of mercury.

The lower end of the glass tube is filled to about one inch in height with a silk thread, *e*, to prevent dust from entering. Great difficulty has been experienced in procuring tubes of uniform taper. Experiments have been made with about 50 tubes, and only three of them were found to answer the purpose. In the best one of the three, the column of mercury had a uniform range of 48 inches for a difference of 2 inches in height; that is, as 24 to 1. This tube was accidentally broken; of the remaining two, which are now in use, and which were exhibited to the meeting, one has a range of 16 inches between 29 and 30 inches barometer height.

As the tube must necessarily be of very small diameter, the capillary attraction has a great influence on the real height of the column of mercury. In the tube of a mean diameter of 0·07 of an inch, when a height of 30 inches is indicated, on comparing with a standard barometer, the real height of the column is found to be only 28½ inches; and in the tube with a mean diameter of 0·06 of an inch, an indicated height of 29⅜ inches is equivalent to 30 inches of a standard barometer: so that for the same pressure of the atmosphere, the smaller tube carries a higher column. It might be supposed that some air remains in the tube above the mercury which reduces the column; but such is not the case, as may be readily shown by allowing the column to move up to the top of the tube, where such air would be detected.

I have had this instrument in my room since January last, about nine months, and have always found it correct and delicate. In stormy weather, I have seen the mercury rising and falling several inches in one minute, indicating the waves of the air ocean above us.

The same principle could also be applied to manometers for experimental purposes in delicate measures of densities of gases.

Mr. Nystrom also showed several specimens of improved T-squares.

Simple as this instrument seems to be, it is difficult to procure one that will satisfactorily answer the purpose for which it is intended. The rule of the square is generally made of hard wood, which is apt to warp, and it is heavy and clumsy to handle on the drawing board. The most suitable wood for a T-square is *spruce-fir*. This wood is not affected by a change of weather, and therefore will not warp; it is among the lightest of woods, its specific gravity when well dried ranging between 0·4 and 0·5, but it is too soft to be used against the drawing pen, in consequence of which, it is necessary to line the edges of the rule with a harder wood. It is of great importance with what kind of hard wood the spruce-fir rule is lined. Most of the hardest woods are not suitable for the purpose, as they warp and twist the rule, and in some cases will separate from it. Maple is found to be the best for the purpose. It is a fine grained, hard wood, and its specific gravity when well dried is only 0·6; its behavior in changes of weather coincides very much with that of spruce-fir, and it is therefore best adapted for this purpose.

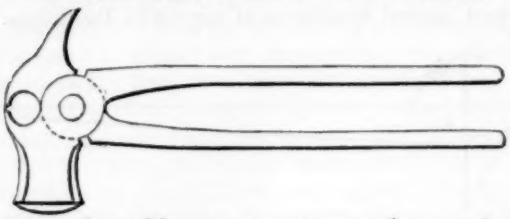
The squares shown to the members were made in Sweden; the one four feet long was made at the Technological Institute, Stockholm, in the year 1845, and it is as perfect now as when first made. The others were made about two years ago.

Another improvement in these T-squares is that the rule, *a b*, is tapered from the cross-piece to the other end; it is also much thicker at *a* than at *b*; this makes the square lighter and more agreeable to handle on the board. The one edge is at right angles, while the other can be set at any required angle by the movable part of the cross-piece. There is also another improvement in the manner of fastening the rule to the cross-piece. The space cut out in the cross-piece for receiving the rule is made fully $\frac{1}{32}$d of an inch smaller than the width of the rule, which latter is sprung in and fastened with four screws. Experience has shown that this mode of fastening the rule is far superior to that of gluing; besides, the rule can easily be taken out when it is required to adjust the cross-piece, which latter is generally made of rosewood.

I have had T-squares of this description for ships drawings, 10 feet long. The rule was 3 inches wide by $\frac{1}{8}$th thick at the cross-piece, and only 1 inch by $\frac{1}{16}$th at the other end. In order to handle such a long

square at the smallest end, a string was fastened at the screw in the cross-piece, extending to the other end, by means of which the square could be moved and set as delicately as if handled by the cross-piece. The edges are beveled down to a uniform thickness of about $\frac{1}{20}$th of an inch.

Mr. Nystrom also showed a combination of Hammer and Tongs, and remarked that, an instrument of this kind is very useful about a house, store, or office. It originated from the inconvenience of traveling in Russia. In one journey from Sevastopol to Moscow, twenty-one days and nights were occupied in continuous traveling; during this time, the wagon or tarantass broke down or got out of order several times, and a hammer and tongs of this description was found to be very useful.



Mr. Howson exhibited a large and beautifully finished model of a Breech-loading Rifled Cannon, invented by Mr. T. M. Adams of this city. It has at the rear end of the bore an opening at right angles to the latter for the reception of a movable breech and a wedge, which are so connected together and to a lever, that by lowering the latter both wedge and breech are lowered simultaneously, leaving a clear opening for the insertion of the projectile from the rear of the cannon into the bore. On raising the lever, the breech is elevated to its proper position at the rear of the bore, and then tightly secured by the wedge.

Mr. Howson also exhibited specimens of the fibres of a plant of the genus Hibiscus which grows in abundance in the marshy lands of New Jersey and other northern States. The applicability of the fibres of this plant to the manufacture of paper, rope, matting, textile fabrics, &c., was discovered by Mr. Cantelo, the well-known inventor of the egg-hatching machine, which was operated so successfully in this country and Europe a few years ago.

Mr. H. also directed the attention of the members present to a Breech-loading Carbine, the invention of B. F. Joslyn, of the Joslyn Fire Arm Company, Stonington, Conn. The peculiarity of this fire arm consists of a tilting breech provided with two conical rings, which are caused to expand by the force imparted to a cone-headed pin, the moment the discharge takes place, thus securing perfect tightness at the breech. The weapon has been submitted to a series of trials at the Washington Navy Yard, where its performance received the commendation of the government officers appointed to inspect arms of this class.

Mr. H. also exhibited an Air-tight Preserving Vessel, invented and patented by J. B. Wilson, Esq., of Williamstown, N. J., and now sold in such numbers as to prove its efficiency.

Mr. Howson also exhibited a beautiful specimen of embossed brass, representing the bust of General McClellan, the dies for forming which were made in this city.

Mr. H. also exhibited a specimen of an improved Ventilated Hat, invented and patented by W. F. Warburton, Esq. The peculiarity of this hat is that it has in front a thin metallic band, which, while it adjusts itself readily to the wearer's head, allows the air to pass freely from below into the interior of the hat.

METEOROLOGY.

The Meteorology of Philadelphia. By JAMES A. KIRKPATRICK, A.M.

SEPTEMBER.—The mean temperature of September (67·95°) was about the same as the average for eleven years, but was two degrees higher than for the same month last year.

The warmest day of the month was the 15th, of which the mean temperature was 75·5°. The 3d was but 0·2 of a degree colder. The highest degree indicated by the register thermometer was 86° on the 3d. On the 15th the highest point reached was 84°.

The coldest day was the 29th, with a mean temperature of 56·2°. The thermometer indicated the lowest temperature (46°) on the 29th. The range of temperature for the month was 40°.

The greatest change of temperature in the course of a day was 26° on the 23d; the least was 5½° on the 18th. The average oscillation for the month (17·22°) was one degree less than that for September, 1860; but was very near the average for the month for eleven years.

The greatest mean daily range of temperature was 16·3°, between the 21st and 22d; the least was 0·7°, and occurred twice, between the 9th and 10th, and between the 13th and 14th days of the month. The average daily range for the month was 4·41°, which was about one-third of a degree less than the average for eleven years, and about three-quarters of a degree less than for September, 1860.

The pressure of the atmosphere was greatest (30·343 ins.) on the morning of the 30th, and least (29·283 ins.) on the evening of the 27th. This minimum pressure is the lowest indicated by the barometer for this month in the last eleven years; the nearest approach to it was in September, 1859, when the minimum pressure was 29·338 inches. The average pressure for the month was three-hundredths of an inch below the general average, and six-hundredths of an inch less than the pressure for September, 1860.

The greatest mean daily range of pressure was 0·426 of an inch between the 28th and 29th; the least was 0·014 of an inch between the 1st and 2d days of the month; and the average for the whole month was 0·144 of an inch, which is a little greater than the average for the last eleven years, though almost identical with the range for September of last year.